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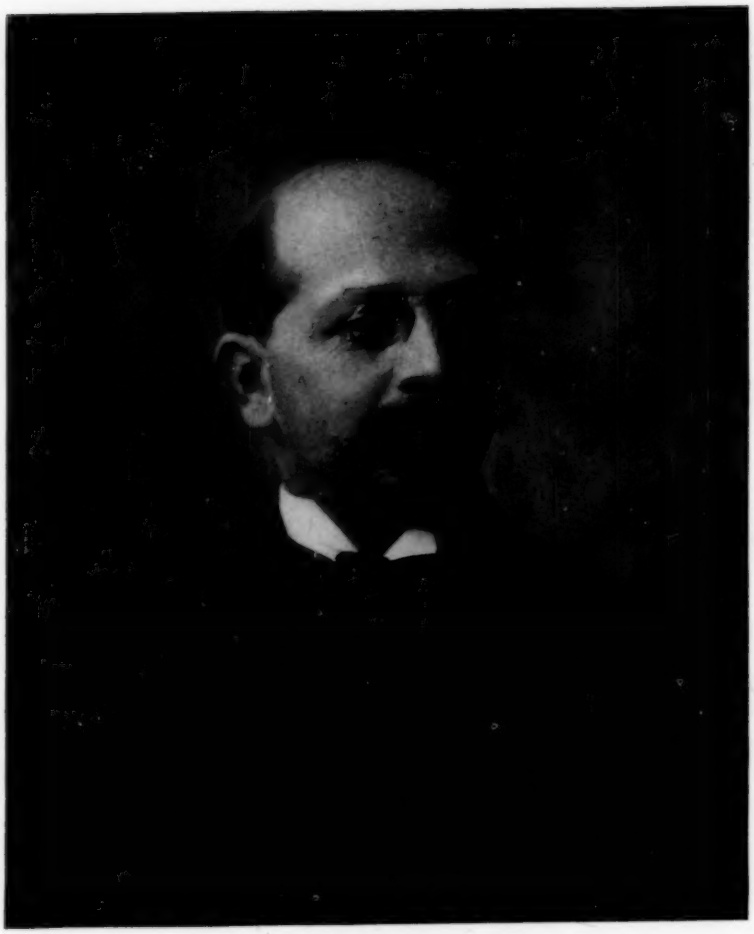

Joseph Jastrow

VOL. 51, NO. 5 SEPTEMBER, 1944

THE PSYCHOLOGICAL REVIEW

JOSEPH JASTROW

1863–1944

In January, 1944 two pioneers in psychology disappeared from the scene, Joseph Jastrow and James McKeen Cattell. In a sense it marks the end of an era. They were the last survivors of the generation that introduced scientific psychology to America. A possible exception is Dewey, for Dewey, although avowedly a philosopher, greatly influenced psychology. Jastrow, Cattell and Dewey were students together at Johns Hopkins in 1882–83. All were in philosophy. Jastrow changed to psychology in the middle of the year when Stanley Hall came as professor. Cattell left for Leipzig at the end of the year. In 1886 Jastrow received the first Ph.D. to be given in America specifically in psychology.

Jastrow was born in Warsaw, Poland, January 30, 1863, the son of a rabbi. A brother, Morris Jastrow, was a distinguished semitic scholar and was for long Professor at the University of Pennsylvania. Joseph Jastrow was an A.B. of the University of Pennsylvania, 1882, A.M., 1885. At Johns Hopkins he seems to have been most influenced by C. S. Peirce. Peirce was on the staff of Bureau of Standards and also lectured in logic at Johns Hopkins. He was a versatile genius who exerted a profound influence upon his students and associates, including William James. The university was a stimulating place owing to the influence of Stanley Hall and of the selected students who were attracted by the first graduate school in America. Jastrow was Fellow in 1885–86 and when a situation did not open, continued as honorary fellow until 1888.

During the six years at Hopkins Jastrow was industrious in research and writing, publishing eighteen papers of widely different character during the period. The first was a joint paper with Peirce, 'On Small Differences in Sensation,' published in the Memoirs of the National Academy in 1884. It repeated the Weber experiments on passive pressure, using a pressure balance. The results were in harmony with the early experiments, of course, but it was argued that one had no right to think of the limen of sensation as a discrete unit. Since the number of right judgments increased with the growing difference between the weights in accordance with the least squares and the laws of error, Peirce and Jastrow argued that it was misleading to speak, as Fechner was inclined to speak, as if the limen was a definite entity. Jastrow continued his interest in the Fechner phenomenon as is shown in his article in the *American Journal of Psychology* in 1887, 'The Psychophysic Law and Star-Magnitudes,' and 'A Critique of Psychophysic Methods' in 1888.

Jastrow's dissertation was 'The Perception of Space by Disparate Senses,' published in *Mind* in 1886. It showed that vision was dominant over touch

and kinæsthesis where the three came into conflict. Galton's influence was shown by studies in composite portraiture, in the precocity of genius, in the longevity of genius, in the dreams of the blind, and in a study of eye-mindedness and ear-mindedness. Other studies were on rhythm and a new method of measuring reaction times, the chain reaction. Jastrow also showed at this time the beginning of what was an important interest of his life, the psychology of deception. The next six years gave evidence of a great versatility in interest and great persistence in execution. Several of the papers which foreshadow his skill in popularization were published in the journals of wider circulation.

Jastrow was appointed Professor of Psychology at the University of Wisconsin in 1888, the same year that Cattell began his work at the University of Pennsylvania. This was one of the first chairs in psychology in America and the first west of the Atlantic seaboard. Jastrow at once established a laboratory. The apparatus was largely hand-made, although Jastrow asserts that an order from him started the now well-known Stoelting Company on its program of constructing psychological apparatus. The experimental course was dependent upon actual instrument work by the students. It was a laboratory course paralleling a series of lectures on principles. Stress was laid from the beginning upon research and frequent articles were published by students under Jastrow's direction in the early numbers of the *American Journal of Psychology*. In addition to courses in psychology Jastrow taught courses in logic, in Greek philosophy and in aesthetics. Aside from aesthetics these courses were given up in a few years. Jastrow developed an interest in aesthetics that induced him to continue with it over the years.

After a year of lecturing, Jastrow felt that he needed greater first-hand knowledge of the work that was being done in Europe and asked for a leave of absence. He spent the year travelling and visiting the universities of Germany, France, Italy, and Great Britain. He presented the results of his study in three articles published in the Open Court, each a summary of the work that was done in one or two of the countries. The articles were brought together under the title of 'Aspects of Modern Psychology,' published in a volume, *Epitomes of Three Sciences, 1890.*

Jastrow was asked to take charge of preparing an exhibit in psychology for the Columbia Exhibition in Chicago in 1893. He made it in part an exhibit of apparatus, but added an element of interest by giving the mental tests of the day to the visitors who desired them. They attracted large numbers of visitors and gave a wider knowledge of what psychologists were doing. The only available tests at that time were after the pattern of Galton. Although many records were made, they were not published or otherwise interpreted.

The work of the laboratory at Wisconsin was at first devoted to the investigation of various phases of sensation. One of the most quoted studies was of involuntary movement that was reprinted in *Fact and Fable in Psychology,* 1900. Studies of illusions, some very ingenious ones original with Jastrow, were also made and reported in the same volume. Investigations of stereoscopic vision loomed large in the list. Association as seen in community of ideas was also investigated as was apperception in its relation to the periphery of consciousness. Another group of problems was related to the reasoning processes. Always a favorite topic with Jastrow under many guises was the conditions of false conclusions. These were first investigated in the

laboratory, and then in later books were related to popular fallacies. On the other side Jastrow reported on the method of forming judgments, both with reference to the order of merit method and as seen in the ways juries reached their conclusions.

In the early years Jastrow had no help in his work and all the mechanical details, even, had to be taken care of by him. Later, as the number of students increased, he had assistants, first under his own close direction, then for more responsible roles. Dr. Hull came as an assistant in 1915, became an instructor the following year and took over more and more responsibility for the laboratory until Jastrow withdrew entirely from the course in 1919.

Jastrow never enjoyed buoyant health. After the work of superintending the psychology exhibit at Chicago, he reported that he had to follow each lecture by several hours of rest. He sought full recuperation in 1898 in a vacation in Europe. The first part of the period was spent in Oxford writing articles for Baldwin's *Dictionary of Philosophy and Psychology*. The remainder of the year was devoted to the art museums of the continent in furtherance of the work in aesthetics that he made a major phase of his teaching. An operation of a minor character required a long period of convalescence, during 1922. This and the following year were saddened and complicated by the long illness of Mrs. Jastrow. She was Rachel Saold, whom he married in Baltimore in 1888. Mrs. Jastrow had been unusually sympathetic and helpful socially and professionally. Her long hopeless illness contributed to his own difficulties. When she died in 1926 he felt that he could no longer carry on. He took a leave of absence for the following year and resigned his professorship in 1927.

Then began a new life in most respects. Jastrow was appointed to the Staff of the New School of Social Research in New York and also gave increased time to the popularization of psychology, an activity that had always loomed large in his interest. His central activity was a syndicated column in many newspapers headed 'Keeping Mentally Fit.' Mental hygiene was the central theme, but he took occasion to discuss many of the more popular aspects of psychology. He also broadcast on the same topics for a period. Much of the material was reprinted in books, and many other popular books were written in the next dozen years. One frequently reprinted had the title of the column, *Keeping Mentally Fit*, 1928.

Most of Jastrow's books are collections of periodical articles or are closely associated with his lecture activities. Aside from those mentioned earlier, *The Subconscious*, 1906, was suggested by the studies of F. W. H. Myers' *Human Personality*, and related studies in psychical research, and the work of Prince and others on double personality. It gives a reasoned discussion of relations between full consciousness and various aspects of the less fully conscious, both normal and abnormal. An invitation to give a series of lectures at Columbia in 1910 gave rise to two volumes on the more emotional aspects of mental life: *The Qualities of Men*, 1910, and *Character and Temperament*, 1910. He regarded the former as a briefer and more literary treatment, the latter as the more scientific. They provide a summary of the psychological knowledge of the period, together with much in the way of suggestion of new views and common sense advice.

Another favorite topic with Jastrow was the credulity of human nature. The expressions of this phase began in *Fact and Fable*, and dominated the *Psychology of Conviction*, 1918, *Wish*

and Wisdom, and the later books. They discussed the various mediums and their exhibitions and theories with a refutation of their claims to a higher revelation. Jastrow, with Houdini and Coover, took the negative in Murchison's Symposium 'The Case For and Against Psychical Belief.' Jastrow's contribution was a logical argument against the possibility of the different manifestations having the significance that was assigned them. He also questioned the adequacy of the precautions against deception. The books of the later years: *Piloting Your Life,* 1930; *Effective Thinking,* 1931; *The House that Freud Built,* 1932; *Sanity First,* 1935; *Life of the Mind,* 1938; *Betrayal of Intelligence,* may be mentioned as amplifications of the principles of hygiene, and of appeals for clear thinking. They had a wide appeal and were marked by common sense and full knowledge.

Jastrow can be said to belong to no school. He began his work before the modern schools had been recognized. In that period psychologists were divided into the philosophical or armchair group and the experimental. Jastrow belonged to the experimental. He took little part in the structuralism-functionalism controversy. Much the same may be said of his reactions to behaviorism. The objective approach appealed to him, but he regarded Watson's explanations as too limited. In his autobiography he characterized Watson's conclusions as "based on data, remote, misinterpreted and wholly inadequate."

He gave a similar recognition to Freud's views. In his *House that Freud Built* he recognizes the importance of memories in the causation of disease, but he is sceptical of the bizarre framework and improbable myths that Freud imposes upon his disciples. He speaks of the fallacy of universal sex causation of disease, and of the fantastic constructions. He is even doubtful that psychoanalysis is the only or even the best means of treatment. Jastrow did not discard the real contributions of Freud, but gave a limited adherence to the 'system' as a whole.

Much the same attitude was taken by him toward the Gestalt school. He spoke of it as compensating for the "nuclear approach of the analytic psychology." He did not, however, become a devotee of the entire system.

Jastrow must be regarded as an eclectic in the sense that he was open-minded to the best points of all schools, but an exclusive protagonist of none of them. He read each critically and applied what seemed essential to an understanding of mind as a whole. He thought of himself as developing a naturalistic psychology. This he regarded as having the primitive aims of behaviorism with none of the excesses and arbitrary limitations of method and interpretations that he thought constituted defects in Watson's work. He also speaks at times of his psychology as having a genetic or more generally biologic basis. None of the formulations of his theory were distinctive enough from those generally current to cause them to be attacked by others and so he did not stand out as a leader of a school. Whether failure to be attacked is to be attributed in a case of this kind to lack of originality, or merely to being sufficiently lacking in extravagance of statement to escape open contradiction, may be a matter of opinion. In Jastrow's case it can, I think, be definitely ascribed to keeping the middle of the road, restricting his statements to those that can be easily justified and generally accepted. However we class the position in general, it marks Jastrow as an eclectic rather than the follower of any one school.

Jastrow stands as a worker high

among the early list of investigators. His interest in topics of the abnormal and in popular delusions took him away from the specifically laboratory problems relatively early in life. For that reason and because of repeated periods of poor health, his purely experimental contributions were fewer than they might have been. His skill in presentation of material both in speech and writing in a way to appeal to the general public led him to devote a great portion of his time and talent to popularization at a dignified level and in the best sense of popularization. He was for years one of the most acceptable men in the presentation of material to a popular audience. In his earlier books the appeal was to psychologists and the enlightened public. In the later books and articles the shift was toward an appeal to the less enlightened, and took the general psychological results for granted. Even in the later years the matter was kept at a high level for the group he aimed at. There was no exploiting just for the sake of sales or publicity. Jastrow always left a dignified impression of psychology and did nothing to bring the science into disrepute. On the other hand he did much to extend its appreciation in other than academic or scientific circles.

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PSYCHOLOGISTS' MISUSE OF THE AUXILIARY CONCEPTS OF PHYSICS AND MATHEMATICS [1]

BY IVAN D. LONDON

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I. Introduction

A number of auxiliary concepts must be employed in every science; for example, in physics the atom, energy, force and matter are elemental concepts out of which the theorist builds the structure of his science. Concepts of this kind may often be applied to the description and understanding of observations which superficially bear no resemblance to the original phenomena for which they were constructed. There is the ever-present danger, however, that in a foreign context these concepts may be fatally distorted and lose point as has been indicated by a succession of theoretical physicists and mathematicians (8, p. 38; 10, pp. 45–47; 22, pp. 96, 127; 27, pp. 108–111).

Contemporary psychology is liberally provided with examples of both kinds of auxiliary concepts—modern use of *frame of reference, continuum, matrix, dependent* and *independent variables, resistance, gradient vector,* and *psychological force* are widely employed with varying success. It is not our purpose here to discuss the values to be derived from the uses of these more isolated examples, but rather to examine in detail one psychological system which is today foremost in the use of physical concepts applied to the solution of problems outside their explicit domain—the psychological system of Kurt Lewin which has come to be known as topological psychology.

[1] This paper developed from a consideration of the above topic in a seminar conducted by Dr. Willard L. Valentine at Northwestern University during the summer of 1941 and was continued by the author as an independent investigation.

II. The Nature of Force

Lewin employs *force* as a dynamic construct, a view which he holds to be fundamental to his theories (19, pp. 71, 87). This is a view which reflects a Newtonian conviction but is not at all in accord with theory in modern physics, as in modern physics force is geometrical in character.[2] The following discussion will serve to make this distinction clear.

Force in Classical Physics: According to Newton, a free particle moves uniformly in a straight line when left to itself. If it either deviates from its straight path or changes its speed (accelerates), Newton supposes that an external agency is responsible for its failure to maintain the same speed or to follow the prescribed initial path. He does not ask why a particle should move in a straight line. He states only that when a particle does deviate from this straight line, it must be because an agent either pushes or draws it from its course (repulsion or attraction); or, when its speed changes, it must be because an agent either pulls at it (positive acceleration) or pushes against it (negative acceleration). This external agency Newton terms "force." There is a marked flavor of anthorpomorphism in the concept, to be sure, but, as Newton formulates it, it is a dynamic construct (4, p. 38).

Force in Relativity Physics: The approach of the relativists to the problem

[2] In quantum theory force is actually statistical in character, as the effect of interactional forces is equivalent to the expected statistical effect arising from the indistinguishability of the ultimate particles which physics has created (5, p. 128).

of free motion in space is different and fundamentally so, for it is not the straight line, but the geodesic which the free particle pursues in space.[3] The geodesic designates the shortest line that may be described between two points in any space. In Euclidean space the geodesic corresponds to the familiar straight line—the shortest distance between two points in ordinary experience. However, this is not the case in non-Euclidean spaces, for in these spaces geodesics seem to take on curvature (11, pp. 153–154).

A certain non-Euclidean space is the basic space of relativity. Its geodesics are not Euclid's straight lines. Accordingly, the relativist ascribes the apparent deviations from the straight line that free moving particles show directly to the geometry of the non-Euclidean space employed. In other words, no external agency like force is needed to account for the observed curvature away from the straight path, as the geometry alone is made to take care of the situation simply by the assignment of an appropriate geodesic.

The laws of motion which require the free particle to traverse the geodesic have the simple form $\delta \int ds = 0$. The particular space in which this is possible is called *natural* space. However, the laws that govern the course of a particle in Euclidean space do not have this simple form. To account for the observed motion, they require in addition the inclusion of forces. Such a space is called *abstract* space by writers on relativity theory (4, p. 38; 22, pp. 361–362).

Force is, then, to be looked upon as generated from the discrepancy which exists between the geometry of the natural space in which the particle moves and the geometry of the abstract space to which the motion is arbitrarily referred. Newton erroneously assumes that the Euclidean geometry of our everyday experience is the only geometry of physical space. He is obliged, therefore, to invent the concept of force to account for the otherwise inexplicable deviating behavior of the free particle from the straight line. The relativist, on the other hand, refers the entire matter to the geodesic of non-Euclidean space and finds force a superfluous and irrelevant concept. Because of its geometrical genesis, force is a geometrical construct and in relativity an unnecessary one (4, pp. 37–38; 6, pp. 43–47, 82–85; 22, p. 358).

Lewin's Use of Force: Relativity and quantum physics render untenable Lewin's position in regard to the necessary dynamic character of the construct, force. They remove a basic assumption and thereby weaken seriously the subsequent structure of his theories. Moreover, as a dynamic construct, force is basically an anthropomorphic concept, for an agent is made responsible for variations in behavior. No amount of mathematical disguise can hide this fact. Such a construct has ultimately no place in a science which wishes completely to eliminate all anthropomorphic elements from its body. Yet, Lewin utilizes, without methodological qualms, a construct which is fundamentally anthropomorphic (27, p. 46).

Force represents an easy way out of difficulties; for, if it is impossible to account for an item of behavior, it is always possible to invent an agent to cause it. In this sense, Lewin's psychology abounds in the too facile erection of force fields. That Newton is guilty of the same procedure does not justify Lewin's practice in this regard,

[3] The geodesic has an equation of the form $\delta \int ds = 0$. This states in the language of the calculus of variations that the path described in space by a moving particle must be a minimum; that is, $\int ds =$ an extremum, where ds is an infinitesimal distance element of the space that is characterized by the given geodesic (1, p. 144; 10, p. 116; 22, p. 362).

for Newton places rigorous limitations on *his* agent in his basic assumptions and devises a sharply formulated mathematical harness to keep it in bounds. There is in Lewin's works nothing that approaches even remotely such curbing despite a constant insistence to the contrary (4, pp. 96, 189; 9, p. 215; 16, p. 20).

III. 'ARISTOTELIAN' AND 'GALILEAN' MODES OF THOUGHT

Lewin distinguishes two modes of thought in the history of science. One of these he dubs 'Aristotelian'; the other, 'Galilean.' He sets them in mutually exclusive opposition to each other; extolling the latter, condemning the former. Although these two categories of Lewin's will be discussed more fully later on, the following observations should be made, following as they do on the heels of the discussion of the nature of force:

(1) According to Lewin, the Aristotelian mode of thought tends to use valuative concepts; whereas the Galilean mode of thought insists on concepts that are non-valuative. To be sure, Aristotelianism abounds in valuative criteria. But Aristotle's perfect circles and orbits find their counterpart in the perfect straight lines that Newton's particles theoretically traverse. Owing to the constant action of gravity those straight paths never show up in actual nature. They exist only in Newton's mind as a valuative criterion and basic assumption. Even the minimal idea implicit in the geodesic is valuative. Physical ideas like 'the shortest path in space' are on a par with 'the most perfect orbits' of Aristotle; in this particular case because the shortest path here becomes *per se* the perfect path, geodetic motion being the perfect motion of a free particle in space. Mathematical assertion may take the place of previous verbal statement; but valuation remains in disguise. There is no basis in physics for Lewin's extreme position in regard to the role of valuative criteria (2, p. 40; 22, p. 116; 10, pp. 104, 152; 22, pp. 88, 116).

(2) Lewin condemns the statistical derivation of laws as Aristotelian. For him statistics can have no explanatory value, can form no laws. Yet the effect of interactional forces may be derived statistically. Lewin is evidently unaware of the extent to which statistics dominates quantum theory and even much of classical physics, as in the kinetic theory of gases. Since statistical methods play powerful roles in post-Galilean physics, Lewin's position regarding them can be regarded as definitely mistaken and misleading (21; 27, pp. 24, 50–52; 2, p. 38; 18, p. 10).

IV. HODOLOGICAL SPACE

The concepts of directed locomotions and vectors in topological space which is directionless in essential character are mathematically inconsistent. Lewin attempts to get around this inherent difficulty by the creation of a peculiar kind of space characterized by certain directional properties. It has not been mathematized yet,[4] and it is doubtful whether it ever can be (2, p. 60; 16, p. 34; 19, pp. 22–23; 14; 24).

Lewin's aim is to construct a comprehensive psychological field theory. In pursuit of this aim he tentatively drafts and projects for the future a hypothetical space in which force functions as a supplementing and separate construct. The most successful field theory to date in any science is the theory of relativity. However, its verdict on the geometric character of the

[4] J. F. Brown (2) seems to think its bases are mathematically sound. Lewin, himself, expresses only the hope that future mathematicians will find it so (2, p. 61; 19, p. 3).

construct, force, has been overlooked by Lewin, so that force, a property and manifestation of space, has not been made by him to stem from hodological space, but to function there as an extraneous dynamic construct (19, p. 96).

Had Lewin recognized the geometrical nature of force, the impossibility of a hodological space such as he intended would have been manifest, for then so many additional requirements would have had to be imposed on the space that it would have been impossible to expect any geometry to be developed adequate to meet the additional demands. Even before its improbable mathematization it would have had to be abandoned because of the conflicting requirements placed on it, and suffer the fate of the ether concept which for the same reason was given up years ago (3, p. 250).

Furthermore, the whole initial procedure is questionable. Spatialized psychological properties are assigned to a hypothetical space with great mathematical indiscrimination, despite the fact that until the requisite geometry is produced in detail there is no guarantee —in fact, little likelihood—that such a mathematical group of coexisting properties can exist. It is highly improbable that any sort of geometry can be constructed to conform to so many different specifications prescribed in advance, because it is difficult by mere inspection to detect implicit contradictions in a given set of assumptions. To study, as Lewin does, the properties of a new kind of space without first having mathematically formulated the necessary geometry which expresses them is one of the most futile of enterprises. Vector psychology is fundamentally unable to claim a solid deductive character because there is no geometric machinery for handling the deductions (3, p. 21; 12, p. 5; 18, p. viii).

V. THE NATURE OF CAUSALITY

A basic determinant of the form into which Lewin shapes his theorizations is his conception of physical causality. In order to discover whether Lewin's conceptions on this important matter really conform to their physical prototype, it will be necessary first to investigate the role of the principle of causality in physics.

State and Causality: In physics the term 'state' is so defined as to cover the two aspects of a physical system: the totality of its perceptible properties (Lewin's phenomenal properties) and those numerous properties that indicate how any system which possesses them would behave when brought into certain experimental situations (Lewin's conditional-genetic properties). Having defined 'state' thus, it is possible to formulate the principle of universal causality, as understood in present-day physics, as follows: if a state A *of the universe* is once followed by a state B, whenever A should occur, B will follow. It remains only to provide the principle with operational significance to allow its incorporation into science (5, p. 297; 10, pp. 5, 9; 22, p. 90; 18, pp. 19–20).

The principle of causality cannot be applied to just a part of the universe. It must be applied to the whole of the universe in the totality of its aspects, or not at all. This, however, makes an empirical test of the principle or an operational formulation of it impossible, because one can never know the state of the whole universe at any one given time in all its aspects. Furthermore, it is not at all certain whether it is possible for a state A of the universe ever to return. For example, the factor of entropy, which measures the unavailable energy in any thermodynamic system, forbids the possibility of a state A ever to return because of the irreversi-

bility of the processes involved (10, pp. 18–19; 27, pp. 94, 111).

If one attempts to determine the state A of the whole universe at any given time in all its aspects, a disconcerting situation arises; for an experimental determination of one part of the universe (that is, of some of its aspects) results inevitably in a local disturbance of its state. This, in turn, renders immediately an unavoidable change in the total state of the universe since the whole is indissolubly linked to its parts. Since any experiment is nothing more than a planned, methodical disturbance of states, there is no method of getting at the state of the universe that does not at the same time tamper with it. The determination of state A, so essential for the meaningfulness of the causality principle, is then an impossibility. Operationally, the law of causality, which defines the recurring sequence—if A, then always B—turns out to be in physics a dead letter (27, pp. 21–22, 68–69).

The exact law of causality cannot find application in physics; only a formulation of it which says something approximate can. The latter asserts that, if in a finite theoretical system state A is at one time followed by state B and at another time by state C, the system can be enlarged and modified sufficiently so that an ascertainable and deducible state C may be made as close to a like state B as one pleases. The law of causality turns out to be a limiting case which the law for finite theoretical systems approaches as the systems are made larger and larger, maintaining for itself merely an ideal existence (4, pp. 82, 85–90; 8, pp. 19–21).

If the law of causality is not obeyed according to a particular definition or formulation of the state of a system, one can redefine or reformulate the state so that the law is obeyed. As a science progressively advances from less satisfactory theories to other more general and successful ones, this is exactly what is done. However, if such is the case, it is evident that the causality principle does not state a fact. It merely defines the term 'state.' Neither experiment nor any amount of abstract reasoning can then prove or disprove the law of causality; and, not because it is a truth known *a priori*, but because it is a purely conventional definition of what is acceptable of a state and its correlative theoretical system. It can be valid only, if on the basis of known operational laws it can predict the succession of states when complete information is available. This conception of causality is positivistic and dominates present-day physics (5, p. 297; 10, pp. 18, 22; 22, p. 519; 28, p. 113; 27, pp. 52–53, 73, 82).

Causality and Dynamic Enforcement: Physical laws are nothing more than symbolic descriptions of observed regular successions of states in limited fields of phenomena. They provide no reason for any of the states encountered; neither do they constitute what is popularly called an explanation. For example, Newton's law of gravity is not an explanation of gravity in the sense that it explains *why* particles attract each other. The law merely gives an exact description of the observed apparent attraction and nothing more. As another example, Hooke's law for springs states that within the elastic limits of the spring extension is practically proportional to the force associated with it. Popularly, the force is said to cause the extension; but, as far as Hooke's law itself is concerned in its symbolic form, it is just as sensible to call the extension the cause and the force the effect as the other way around.

All that laws state are relations among symbols which represent well-defined operations in the laboratory. They involve no notions of precedence or of dynamic enforcement. In classical

physics the ordinary description of nature in terms of space and time is indisputably *acausal,* so that there the law of causality is a concept in reality extraneous to the science which manages to do a superb job without its assistance (22, pp. 19, 517, 520, 527).

Total Life-Spaces: The discussion relating to the impossibility of determining the state A of the universe in the totality of its aspects at a given time can be applied directly to Lewin's problem of devising the momentary cross-section of total life-spaces. The same difficulties must crop up, for experimental determinations of any portion or region of total life-spaces must in the very process automatically change them. *Successively* determined parts cannot be additively glued together to present a previous *simultaneous* dynamic picture of things, or, as Lewin puts it, its momentary cross-section. Though the connection is unorthodox, this is good Gestalt doctrine (5, p. 104; 29, p. 97; 31, p. 69).

Causation According to Lewin: It should be evident from the preceding discussion that Lewin's conception of physical causality does not reflect its prototype in physics. Lewin differentiates between two concepts of causality —a 'systematic' and an 'historical' concept of causality. The question, "Why does a given state A have a state B and no other as a result?" exemplifies the former. The question, "Why does just such a state come into being?" exemplifies the latter. It will be noted that in both 'systematic' and 'historical' causation, the 'why' dominates the question.[5] This is at complete variance with theoretical practice in physics, for physical laws attempt to answer the question, "How?" rather than the question, "Why?" It is the correlation of states that is sought after and not causative explanations—explanations which involve always the notion of a dynamic enforcement (2, p. 41; 18, p. 30).

Even granting the states A and B to be determinable in total life-spaces, Lewin's preoccupation with the 'why,' instead of with the 'how,' vitiates the possibility of any derived support from physical causality. In addition, Lewin's conceptions of psychological causation forfeit the chance of physical validation because of the notion, so essential to him, of an explanatory dynamic enforcement which is implicit throughout his thinking (18, pp. 20–21, 30, 45; 19, p. 71).

Lewin, abjuring 'historical causation,' strives to make his 'systematic-causative' laws free of specific dates. In this attempt he concurs with good physical usage. But this does not render laws ahistorical in the sense that the past has nothing to do with their application. For example, a certain equation in physics may be free of specific dates, but it will contain instead a number of arbitrary constants. Although implicitly the equation may supply a complete picture of a given situation at a given time, actually it can do so only if the values of the constants are first determined. This can be done only by reference to past situations. For instance, the equation, $x = \frac{1}{2}\ gt^2$, where $g =$ the gravitational constant, has a temporal application that is general. However, before the equation may be used, the value of g must first be determined for the particular locality, as g varies over the face of the earth. It would be wrong to say of this equation in practice that it is ahistorical, for only by reference to a past situation (that is to say, to a past experiment) may the constant g be evaluated for the particular locality in question (18, p. 31; 2, pp. 20, 39, 78).

It is, therefore, misleading to assert

[5] J. F. Brown asserts that to a certain extent in field theory the 'why' is synonymous with the 'how' (2, p. 41).

with Lewin that field theory requires any change x' at a point x_1 to depend only on the total situation at the time t_1. Rather, x' depends on certain aspects of the total situation obtaining at time t_1 (the boundary conditions of physics) and in practice is made to depend on boundary conditions at a time t_2 prior to the time of application in order to bring about the evaluation of the several critical constants involved (20, p. 295).

It has already been pointed out that, whenever there are deviations from prescribed behavior which are unaccountable, an easy way out is afforded by postulating a force to cause the deviations, so that without the action of the force a change could not take place. Energy is made by Lewin to perform functions similar to those performed by force, and as Lewin employs them, causal energies, like his psychological forces, are those same familiar and essentially anthropomorphic agents which at all times obligingly do what is required of them. The epistemological propriety of such a usage of causality is at best questionable (4, pp. 96, 189).

The notion that energy is something which makes everything go and is the source of all activity is an erroneous impression of the function which the energy concept is made to perform in physics. It is incorrect to speak, as Lewin does, of causal energies and of processes occurring only when causative energy capable of doing work is set free, as energy is primarily a mechanical concept (*e.g.*, K.E. $= \frac{1}{2} mv^2$) and care should be taken to keep this in mind. Energy has no meaning when torn from its definitional moorings. Even in physics, when one raises a question about such things as electrical energy and electrical force, *the energy and force concepts have meaning and application only to the extent to which electrical theory can be developed along mechanical lines;* that is to say, by the use of the customary terms of the latter—mass, velocity, and position. Lewin's employment of this particularly tempting auxiliary concept of physics is inappropriate to the matter at hand (4, p. 189; 22, p. 127; 27, p. 109; 18, p. 45).

VI. The Nature of Field

Another basic determinant of the form into which Lewin shapes his theorizations is his conception of physical field. Here again, in order to discover whether Lewin's conceptions really conform to their physical prototype, it will be necessary first to investigate the role of field in physics.

Field in Classical Physics: In early physics the concept of field was not recognized as such. It was merely a visual representation of *how unit test bodies would behave in a given situation at any point in space.* Its role was a comparatively sterile one, and nothing came of it until Faraday recognized its true significance for physical theory; namely, that it was profitable merely to focus one's attention on the behavior of test bodies in the space around an object in a state of physical excitation and that the object itself need not be the subject of direct investigation at all in order to understand the nature of that excitation. In the electrical theories prior to Faraday, it was characteristic for the theorist to concentrate all his attention on a hypothetical electric fluid flowing along a wire and to dismiss the spatial surroundings as mere background. Faraday's new theories diverged sharply from this outlook and reversed matters. No longer was the space surrounding the wire thought of as irrelevant to the understanding of phenomena, but as holding the only key to their eventual comprehension. To Faraday the properties of the field alone became essential for the description of

observed phenomena, while differences in the source of the field no longer seemed to matter (5, p. 40; 22, p. 283; 8, pp. 131, 157; 10, p. 96).

With the work of Faraday and others the use of field representations soon achieved its vindication as having more than mere graphic interest; for strong similarities were uncovered between fields generated by sources, seemingly strongly disparate in kind. For example, a solenoid and a bar magnet seemed two very different objects. Yet, when a current flowed through the former, the resultant magnetic field was found to be precisely the same as that of the bar magnet. This permitted a consequent identity in all the accompanying effects and, hence, a great unification of theory (8, pp. 137–138, 158).

So useful did the field concept become that in the culminating theory of Maxwell, if the field was known at any one instant alone, the equations of the theory could allow a mathematical deduction of how the whole field would change in space and time. By means of a final translation of the visual field representation into the highly compact, abridged notation of the mathematical field equations, the history of any changing field could now be traced at will *without any further reference to the field itself*. This latter development illustrates the decisive fact that in physics all fields that possess any *deductive capacity whatsoever and freedom of development away from their visual representations* are always characterized by values associated through rigorous mathematical linkage with certain physical quantities *at every point of the field at every instant of time* (8, pp. 152, 157; 22, pp. 287, 302).

Field in Relativity Physics: In relativity the concept of field comes to complete fruition. Field here is no longer a spatial concept in the old sense of the term, but by a treatment of time specifications as space coordinates it is transformed into a concept that is spatial only in a mathematical sense, for the field is incapable of visual representation. As a matter of fact, in relativity field theory the spatial and temporal aspects of phenomena are united so firmly that both space and time perish in the union and only a space-time survives the fusion (1, p. 21; 22, pp. 346, 377).

A consequence of this new conception of field is that field becomes preeminently a *static* concept. For instance, motion is no longer represented as something which changes in a three-dimensional space continuum, but as something which exists in a four-dimensional space-time continuum in all its temporal and spatial aspects. The past, present and the future of that motion are made to exist simultaneously in a static field. Consequently, the motion of a particle appears as a curve imbedded in a four-dimensional space. This curve bears the name of 'world line' or 'world path' of the particle and marks out its successive positions in space-time; that is to say, marks out the successive 'now' 's of an observer stationed on it (1, pp. 23, 154; 8, p. 217).

In this scheme each observer carries around with him his own system of reference, or set of axes, to which he refers all world events. For this observer the time-axis is always tangent to his world path, and for him the 'now' of the universe with all its events is always the three-dimensional cross-section of the space-time continuum taken perpendicular to his time-axis at the very point where he is momentarily stationed. The flow of events [6] for an observer, then, is pictured as an infinite series of world cross-sections, taken

[6] The term *event* is applied to the point of intersection of the world cross-section with a world path embedded in the space-time continuum.

perpendicular to his world path, across a static space-time field (1, p. 125).

The static field conception of relativity saves it from the acausality of classical physics, for space-time in physics is constructed largely by means of causal laws which go on the assumption that, if there is a causal law connecting two events at different places in space-time, they are connected by means of a chain of events in the intermediate places. This chain of events is exactly what is termed the world path or world line. An event, then, in relativity is a point in a static space-time continuum which is connected with prior and future events by means of world lines. It does *not* result from the dynamic interaction of physical factors, as Lewin would aver, but from the intersection of an observer's world cross-section with the world lines embedded in the static four-dimensional space-time continuum (18, p. 33; 30, p. 427).

Physical Field and Simultaneity: The question raised in psychological field theory as to whether "only the present is allowable to have effects in the present" in sound theoretical systems is neatly answered by relativity.

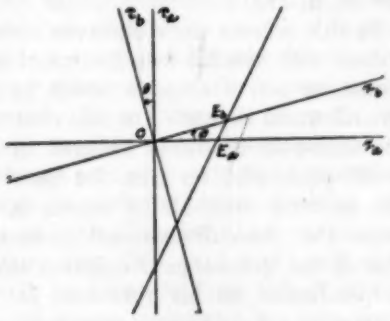

FIG. 1

Generalizations ignoring its answer can be highly misleading (16, p. 36; 18, p. 34).

The world paths of two observers, A and B, moving with *uniform* velocity with respect to each other in a space-time continuum form an angle θ with each other. The two world cross-sections, x_a and x_b, of observers A and B respectively, each being perpendicular to his respective world line τ_a and τ_b, must, therefore, be different. It is then readily seen that, if 0 should be an event common to both A's and B's world cross-sections (0, for example, being taken as the point of intersection of the two world lines τ_a and τ_b), what must be simultaneous with event 0 for observer A will not of necessity be so for observer B. Since the corresponding two world cross-sections of the space-time continuum are different and form the angle θ with each other, different cross-sections are made with the world path E_aE_b of an observed particle E, so that in one frame of reference 0 and E_a are contemporaneous and in another 0 and E_b.[7] Paradoxical as this may appear, both observations are correct, because in relativity simultaneity is meaningless without operational reference to both the observer and the observed. It so happens that simultaneity is a function of the relative velocity of the observers and that the greater that velocity, the greater the consequent angle between the two world cross-sections.[8] It is only when the velocity is very small that the two cross-sections in any way tend to coincide with each other (1, pp. 22–25; 4, p. 14; 8, pp. 215–217; 22, pp. 343, 345, 356).

If t should be used instead of τ proportional to it, the Minkowskian diagram assumes the form of Fig. 2, where, because the metric is not the

[7] This is known as a Minkowskian representation and is possible only if the time coordinate is taken proportional to ict where c = velocity of light, $i = \sqrt{-1}$ (the so-called imaginary number of algebra), and t = number of time units as measured by a clock.

[8] θ = arc tan v where v = relative velocity; θ is known as the rapidity.

familiar metric of Euclidean geometry,[9] all the x's are still perpendicular to their respective t's despite the distortion necessitated by graphing on a Euclidean plane. It is evident directly from the diagram that E_1 and E_2 are events simultaneous for an observer A; that

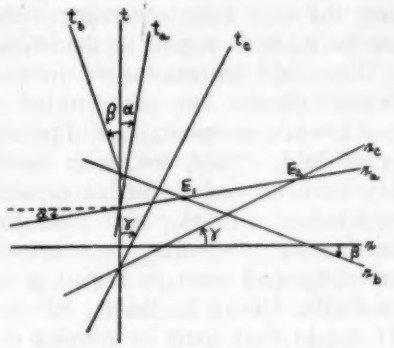

FIG. 2

E_1 precedes E_2 for an observer B; and that E_2 precedes E_1 for an observer C (it being understood that in each case the represented cross-section is only one of a whole series continued upward with the progression of time).

It may now be discerned readily enough that the past does determine the slope of the world lines, for in the past the velocity was set. 'Present' events depend on the observer's velocity and his consequent particular world cross-section. Therefore, the present cannot be disassociated from the past. The past has effects in the present contrary to Lewin's position because the present is only one of many different successive world cross-sections through a static field perpendicular to the variously sloping associated world paths. What is past for one observer may in actuality be the present for another (1, p. 144).

Lewin's discussion of this matter in a fairly recent article is inadequate and misleading to a considerable degree. The diagram set up by him to support his statements is, in fact, actually erroneous. The necessary frames of reference are lacking. The effect of the observer's velocity on the world cross-section is not indicated. For the time-axis t instead of $\tau = cit$ is used. The world-lines in question should even have been made to run differently. For example, Lewin's world-line which produces by intersection with S^t the event x should have been made to run obliquely through the x of the S^{t+dt}, too, instead of being drawn perpendicular to the t-axis. Rectilinear representations of such situations can be adequately handled only by a Minkowskian representation, and this Lewin has not done (20, p. 299).

Universal Physical Field Theory: Field theory in classical physics attained such success and acquired such prestige that for a time the reduction of all phenomena to field formulations seemed to be the goal of all physics. With the discovery of the electron and the development of the electron theory by Lorentz and Larmor, however, the attention of physicists once again returned to the more material aspects of phenomena. This renewed interest in the material aspects of phenomena was naturally accompanied by an attendant decrease of emphasis on the field. But with the advent of relativity the concept of field was again given an exalted position; yet even now its preeminence is restricted to the domain of macrophysics, for in atomic physics field gives way to the probability formulations of an indeterministic quantum theory and becomes there merely a helpful co-worker (22, p. 287; 5, p. 41).

To attempt a reduction of all phenomena to field formulations as Lewin attempts, is to make a big assumption, for there is no *a priori* guarantee that it can be done. Certainly, physics lends no encouragement to the belief in

[9] Metric $\sigma = \sqrt{x^2 + \tau^2} = \sqrt{x^2 - t^2}$ where $\tau = cit$ and c is set equal to 1 for simplification.

this possibility. The two great concepts in physics are still field and matter. No program has been realized or projected which has succeeded in eliminating the latter in favor of the former. As a matter of fact, the whole trend in physics concerning field and matter is toward an amalgamation of the two into a unified field-matter theory, and not toward a universal field theory (5, pp. 41, 48–49; 8, p. 256).

Knowledge of Physical Field: As indicated before, the determination of the state of some fields operationally entails tremendous, if not insurmountable, difficulties; for an effort to investigate one aspect of a field irrevocably changes the whole of it. To get around the difficulty, the physicist, to start with, considers not the whole field, but a very small region or element of it. He asserts that the field *here* and *now* depends on the field *in the immediate neighborhood at a time just past.* If he knows the 'here and now,' the field equations allow him to predict what will happen a little further on in time. He thus increases by small steps his knowledge of the field and deduces what happens here and now from that which has happened far away by the summation of these very small steps (5, p. 100; 8, pp. 152–153).

Psychological Field Theory: Lewin's theoretical position in regard to the concept of field is about equivalent to that held in later classical physics, where the dynamic properties of the field alone appear essential for the description of the observed phenomena. However, in later classical physics differences in the source of the field are of no importance, whereas for Lewin the 'unique case' and 'individual differences' are of great moment. Moreover, for Lewin events in the field must of necessity result from the *dynamic* interaction of psychological factors. The static fields of relativity, however, dispute the theoretical necessity of this view (18, pp. 8, 10, 33; 22, p. 283).

Psychological Field Representation: The field representations of Lewin are about on the same level as those of earlier classical physics without the benefit of the latter's subsequent algebraic and differential counterparts. Indeed, the very same observations that may be made in regard to the efficacy of those field representations in early classical physics may be directed toward Lewin's representations of psychological field. They, too, seem sterile; they have no real deductive capacity, nor freedom of development away from the drawing; they may be interesting pictorially and seem to impart a diagrammatic clarity to things, but have yet to win their spurs by proving their usefulness and indispensability (8, p. 131).

Except for pedagogical purposes, there is in physics no reliance on visual geometry. All its theory can be developed without any recourse whatsoever to figures. The same holds likewise in complete measure for mathematics. As a matter of fact, visual dependence becomes even impossible in the study of the higher spaces of physics and mathematics.[10] In contradistinction to this freedom from visual bondage, 'topological' psychology leans heavily on its diagrams for essential support and has nowhere any independence from them. Field representations in physics, when they do occur, are at the most visualized excerpts or spatial translations from mathematical counterparts. But no matter how geometric the terminology or picturization employed, fields must utilize the symbolic language of mathematical analysis.[11] This should appear to be as true for a real mathematical

[10] *E.g.*, phase-space, relativity continua, etc. (22, p. 219).

[11] The calculus, differential equations, tensor analysis, etc.

field psychology as it is for field physics. Judging from physical precedent, it would seem that Lewin's topology [12] is a mathematics inappropriate to the task required of it (16, p. 14; 4, pp. 42, 75; 22, p. 282; 29, pp. 320–322; 33, p. 16).

Psychological Field Deduction: The equations of Maxwell make possible the mathematical deduction of how a whole field changes in space and time and do so without any reference to diagrams. Nothing like this level of abstraction is approached anywhere in Lewin's field formulations. On that account alone they have yet to earn the designation of genuine field theory. Merely to proceed by statements couched in pseudo-mathematical language as, for example, every behavior B is a function of the total life-space L, which includes both the person P and the environment E, and to conjure up pseudo-formulas like $E = f(L) = f(P, E)$ do not constitute a workable deductive field theory (6, pp. 137–138; 9, pp. 214–215; 16, pp. 33–34; 19, p. 96).

The introduction of symbols does not of itself render psychology mathematical, for their allocation to the various entities or qualities of psychology may represent merely an abbreviation of nomenclature and involve no mathematical conceptions whatsoever. Eddington aptly remarks: "It it easy to introduce mathematical notation; the difficulty is to turn it to useful account," and goes on to illustrate by quoting the following poem from W. J. M. Rankine's *Songs and Fables:*

Let x denote beauty, y manners well-bred,
z fortune (this last is essential),
Let L stand for love—our philosopher said—
Then L is a function of x, y, and z
Of the kind that is known as potential.

[12] So designated because, as will be later shown, Lewin's topology is not the topology of mathematics.

Now integrate L with respect to dt
(t standing for time and persuasion).
Then, between proper limits, 'tis easy to see
The definite integral Marriage must be
(A very concise demonstration).

Pseudo-mathematically the poem states that

$$M = \int_{t_1}^{t_2} \phi(x, y, z)dt,$$

where M = marriage; ϕ (x, y, z) = the potential function of x, y, and z; t_1 and t_2 the proper limits of the definite integral. Needless to conclude, the formula $B = f(P, E)$ is of like spurious character (6, pp. 137–138).

Field theory should be able to describe behavior deductively *at every point in its space at every instant of time.* But in the spatialization of psychology by means of graphic 'topological' representations, Lewin overlooks the precise inclusion of the temporal features of psychological situations. It will not do to assert, as J. F. Brown does, that "although field theory is to be characterized as ahistorical, it is not to be implied that temporal change is of no importance, since what is to be known of time is to be included in the construct of the field" (2, p. 78). It will not do to assert this because the spatializing of time in field makes the field static, as in relativity; whereas for Lewin field must be dynamic. Whatever Lewin's representations may be, they are not equivalent to any of the deductive field formulations of later physics in the accepted sense of the term (16, p. 36).

Psychological Field Determination: It has been pointed out that in physics the field *here* and *now* depends on the field in the immediate neighborhood at a time just past and that knowledge of the field is increased by small steps, so that a deduction of what happens here and now can be made from that which has happened far away by a summation

of these very small steps. Lewin, himself, recognizes that this is the practice in physics, but avers in opposition that in his hodological space direction, for example, must depend not only on the state of the immediate surroundings as in physics, but on the field at large. But simultaneous determinations of the state of the total field in all its complexity are operationally impossible, so that here again Lewin's whole procedure is open to fundamental criticism (19, p. 40).

Ahistoricalism: Lewin asserts that only the present situation can influence present events, since, because neither the past nor the future exists at the present moment, they cannot have effects at the present. He concludes that only what exists concretely can have effects, and that, therefore, in representing life-spaces only the contemporary need be taken into account. This is the ahistorical basis of Lewin's whole approach to psychological field theory. As discussed previously, the answer of relativity by means of the Minkowski representation with its inclusion of time in a static field, refutes this very plausible argument, for in static field theory the past, present, and future exist simultaneously, and the 'now' of the universe (the world cross-section) is a function of past velocities. Furthermore, even in classical physics there are formulations which explicitly incorporate the past to determine the present, as in hereditary mechanics, where boundary conditions, that extend over continuous intervals of space and time, are encountered.[13] Lewin's law of "present effects from only the present" may seem to possess a philosophic vindication, but it has no basis of fact in physical science (5, pp. 297, 302; 18, p. 34).

VII. The Concept of Analysis

Since Lewin is an admitted Gestaltist one should expect to find forcibly asserted the supremacy of the whole over the part, and one is not disappointed. The part-whole relationship is conjured up time and again, so that by its glib application the most stubborn difficulties procure a spurious resolution. While it is true that Lewin's system of psychology represents a departure from preceding formulations of the Gestalt principle, that principle is still its heart and essence. It would seem that so much has already been said by so many people about the part-whole relationship in psychology that any new examination of the question should appear superfluous. Yet a re-examination from the point of view of modern physics may perhaps throw a different light on the subject. Moreover, it would certainly not be out of order since the conclusions of physics do bear directly on matters which Lewin regards as vital to his general approach.

Knowledge and Reality: Lewin asserts that the aim of science is to picture reality, so that the efforts of psychology must be directed toward a comprehension of an objective psychological reality. The modern physicist, however, affirms something totally different; namely, that the aim of physics is not to portray an objective physical world, but to symbolize our knowledge of it. Physics becomes, accordingly, *a theory of knowledge,*[14] and its purpose accord-

[13] For example, when a fiber has been twisted and then released, a knowledge of the state of the twist and the angular velocity of the fiber at any instant is not sufficient for a prediction of its state and motion at any subsequent time. For this purpose the whole history of the fiber since first it began to move at all must be known and incorporated into the requisite integral equations of the theory (22, pp. 55–56).

[14] Psychologists sometimes say that epistemology is the study of 'how' we know, not 'what' we know, but in modern physics 'what' is derived from 'how.' The 'what' is now held to be meaningless unless preceded or accompanied by a statement of 'how.'

ingly cannot be to picture an objective real universe. Modern physics investigates not reality, but knowledge; whereas Lewin, in common with the classical physicist, endeavors to investigate the reality which knowledge is thought to describe. The classical physicist tries to discover a system of equations which will connect *actual* positions and motions of *objective particles* at one instant with their positions and motions at a later time. The modern physicist, on the other hand, attempts the discovery only of equations which will connect the actual *knowledge* of the positions and motions; so that while the classicist can speak relevantly of 'laws governing reality,' the modernist can speak only of probability waves which correlate expected knowledge. Probability is an attribute of our *knowledge* of an event, and not a property of the event itself, which must either occur or not occur. To dismiss, therefore, as Lewin does, the analysis of wholes into parts by affirming that the whole is always greater than the sum of its parts (another way of stating that the parts into which the whole has been analyzed do not correspond to psychological reality) becomes meaningless from the point of view of the modern physicist. Matter, for instance, has been analyzed into a number of ultimate parts whose *real* existence the physicist views with unabashed suspicion (*e.g.*, the electron), but which may serve with varying degrees of effectiveness their purpose in a theory of knowledge (**15**, p. 47; **22**, p. 395; **18**, p. 22; **6**, pp. 49–51, 123; **27**, pp. 52–53, 55).

The Part-Whole Relation: The concept of analysis is fundamental to physics and, for that matter, to any science. This implies the conception of a whole as divisible into parts such that the coexistence of the parts constitutes the existence of the whole. A part must then be thought of as a member of a complete set of parts, and the significance of this part must be thought of as irrevocably bound up with the system of analysis in which it occurs and which serves to precipitate it from the whole. If it performs a useful theoretical function, it need not have a real counterpart and for that latter reason be discarded. The ultimate parts into which physics has analyzed things—fictitious units, if one will—either lurk or are incorporated outright in the equations of physics in order to effect the particular desired connections of observed phenomena. As a matter of fact, the role of these parts in physical theories and their relation to reality are quite analogous to that of complex numbers in mathematics where calculations exist which begin and end with real numbers, but which use complex numbers, that do not have any real counterpart, in the course of the argument (**30**, p. 273; **6**, pp. 118, 134, 145, 164).

Operationism and the Part-Whole Relation: Since wholes cannot be operationally comprehended into a theory, all experience must be dealt with in bits. Physics, for instance, takes the world to pieces and, having done so, then provides itself the task of cementing the world together again. Accordingly, a system must then be devised for reconnecting the parts, and physics sees to it that an interaction between those parts is made to do the trick. The binding material—interaction—is, however, as fictitious as the parts that are bound by it, if one holds to the belief that physics must mirror an objective external world. Furthermore, another implication of the concept of analysis is that for purposes of investigation the parts of the whole are self-sufficient and capable of being conceived as existing without the other parts which abut it. All this is a far cry from Lewin's concept of the 'total situation' which con-

tradicts throughout the doctrine of analysis in that, according to Lewin, parts cannot be meaningfully investigated when abstracted from the total unique situation (17, p. 316; 23, p. 52; 6, pp. 126–127, 134, 204; 7, p. 328).

VIII. Aspects of Law Formation

Let us now consider the question as to whether theoretical science really utilizes in its formation of laws "the total situation in all its full concrete individuality" as Lewin claims, or whether, in opposition, it utilizes identical elements abstracted from a large number of diverse total situations by means of classificatory processes.

Lewin's Stand: Lewin has this to say:

> One method, which one can call abstracting classification, begins by taking into account important single facts and then makes classifications according to one or another of these facts. The individual peculiarities of each situation are thus disregarded. Since there are almost always several such significant facts, such a classification is usually open to attack. It is ambiguous in itself and very often vacillates between opposite characterizations.
>
> In contrast to this the second method begins with the life space as a whole and defines its fundamental structure. The procedure in this case is not to add disconnected items but to make the original structure more specific and differentiated. This method, therefore, proceeds by steps from the general to the particular and thereby avoids the danger of a 'wrong simplification' by abstraction. In such abstractive classification, the second step often destroys the characterization of the first. The 'right simplification' implies a schematization, too, but it is a procedure of 'gradual approximation.' The representation given in the first approximation will not be destroyed but only made more articulated by the second approximation since the whole situation is taken into account from the beginning (18, p. 17).

From Lewin's remarks one would gather that the method of abstractive classification (which, moreover, is not given an accurate statement by Lewin) is taboo in theoretical science. Yet, its application is universal and fruitful even when it results in 'opposite characterizations.' As an instance of this, the phenomena of light may be classified into two categories: those which are wave-like in character and those which are corpuscular. Yet, despite an apparently deep, fundamental contradiction the wave and corpuscular theories of light have enjoyed success—each in its own way. Light is corpuscular in that it has been shown that it travels through space in discrete entities called quanta, and is at the same time wave-like as proven by the experiments on interference and polarization. Yet there is no essential dualism in the description of the phenomena of light, as Lewin would insist, because quantum physics, it turns out, grants no ultimate significance to the terms 'wave' and 'particle.' 'Second steps' in 'abstractive classifications' do not destroy the characterizations of 'first steps' as often as Lewin would imply (22, pp. 389–390, 393; 27, pp. 11, 13, 61–62).

Elementary Abstraction: R. B. Lindsay and H. Margenau in their book on the foundations of physics have this to say about the two methods which Lewin places in antithesis to each other:

> A certain group of sense-impressions were experienced by two observers, who sought to describe the experience by two different ways: the one tried to appreciate the experience as a *whole;* the other, foregoing this, picked out of the whole one small part which he thought he might understand and successfully describe. It is this process of abstraction from the totality of physical phenomena which has undoubtedly been a leading feature in the success of the physical theorist. It is precisely this method of abstraction pushed to its logical conclusion that leads to the use of the differ-

ential calculus in physics. We shall call it the method of elementary abstraction (22, p. 30).

. . . The instantaneous velocity of the fluid at time t at the point P is assumed to be the vector q. This quantity itself is a fruit of the method of elementary abstraction (22, p. 31).

. . . The mathematical process of passing from the differential equation to the physical law is known as *integration*. From the physical point of view the word almost literally conveys the meaning of the method, for just as the differential equation is a symbolic description based on the method of elementary abstraction, the resulting so-called solution is an algebraic equation describing the large-scale operations in the laboratory. Hence, in a very true sense, the passage from the differential equation to its solution involves a symbolic integration of small-scale phenomena into large-scale phenomena (22, p. 35).

These quotations provide an excellent insight into the method of abstraction and undermine several of Lewin's basic contentions. The progressive articulation of an initially undifferentiated whole, which Lewin stresses, simply is not the procedure employed by physics. Physics, rather, employs the direct reverse method—integration of parts into wholes; and the concept of vectors, so important to Lewin for the multitudinous analogies it offers, arises from the very method he rejects—that of elementary abstraction (15, pp. 33–34; 27, pp. 30–31).

The Unique Situation: Closely connected with the question of abstraction in science is that of recurrencies. These are, it must be noted, basically subjective abstractions from objective total situations, since the theorist must first select what he chooses to abstract. Lewin takes this position: the search for regularities alone is denounced as 'Aristotelian' and lacking in penetration, while the unique and the unusual are highly valued by him. Yet, it has for many years been regarded as almost axiomatic that the prime purpose of science is to account for the observed regularities in nature. Theoretical physics would surely never have originated otherwise. Nevertheless, it is true that no situation is ever precisely like another and that rare occurrences will sometimes make an appearance in molar phenomena. In this sense a situation may be "unique in its own full concrete individuality." The question then is: How can this be accounted for (5, pp. 8, 45; 9, p. 215; 15, pp. 18, 30, 35; 18, pp. 8, 10; 6, p. 217)?

Any examination of apparently unrelated situations will reveal that they have in common with each other certain aspects that show elements of regularity, and it is these recurrencies which are the business of science and which are summed up in scientific laws. When situations, however, appear unique or unusual so that no combination of laws can account for them, it is only because they result from a combination of lawful elements and those unavoidably attendant circumstances which are called fortuitous; *e.g.*, the direction and velocity of a sudden gust of wind. It is for this reason that no actual experiment in molar physics ever comes off in strict accordance with law, since every set-up must have its fortuitous elements which, though minimized wherever possible, may not be wholly eliminated by the experimenter; *e.g.*, variations in friction or friction, itself. Laws which themselves are abstractions and idealizations can be obeyed precisely only in idealized experiments (8, pp. 8–9; 27, pp. 49, 52–53).

Lewin, however, stands in complete opposition to this position and in the course of his discussion of the so-called 'Galilean' method remarks that

. . . since the dynamics of a process depends not only upon the object but also, primarily, upon the situation, it would be

nonsensical to try to obtain general laws of processes by excluding the influence of the situations as far as possible. It becomes silly to bring in as many different situations as possible and regard only those factors as generally valid that are observed under all circumstances, in any and every situation. It must, on the contrary, become important to comprehend the whole situation involved, with all its characteristics, as precisely as possible (15, p. 30).

But to insist with Lewin that 'Galilean' concepts prove the need of lawful representations of every "total unique situation in all its full concrete individuality," it is evident, results from the very unorthodox stand that Lewin has taken, and finds no corroboration in physics.

Superposition: According to Lewin, one of the basic concepts behind the construct of force is that force must be linked with locomotion. This, however, is the very Aristotelian notion which Galileo showed had to be discarded—the idea that motion must be connected with acts of pushing, lifting, or pulling and that a moving body comes to a standstill when the force which pushed it along can no longer so act as to push it. The really essential notion behind the construct of force in classical physics is that force must be linked not to locomotion, but to *change of velocity* (that is to say, of direction or of speed, or both), and, in consequence, that force and *change of velocity* (acceleration) must be the vectors having the same direction (8, pp. 6–7, 10, 28; 19, p. 83).

To be sure, Lewin states that psychological force is a psychological construct independent of any construct of physics; and yet he proceeds to use the specialized procedures of physics by relating actual locomotion in psychological life-space to a 'mathematical' totality of the psychological forces acting on a given region at a given time, thus relating locomotion to a resultant force. The idea of a resultant of forces implies the principle of superposition which in turn implies that each component force acts independently of the others. These implications need not be true, but they are justified by their success in practice, since they work satisfactorily in many cases encountered in classical mechanics. Now in Lewin's life-spaces, locomotion is related to the totality of forces. But the use of the concept of resultancy necessitates the reduction of locomotion, as well as of the correlated force, to several disparate components. This use, however, presupposes an operational precedence of the part over the whole, which in view of Lewin's asserted belief in the prior theoretical dominance of the whole is an embarrassing predicament to find oneself in (16, pp. 25, 37; 19, pp. 83, 86; 22, p. 95).

Lewin assumes that psychological forces are causes of change and that, in consequence, laws governing them must be causal laws. Lewin thinks that by *successive* determinations it is possible to ascertain each of the forces and afterwards by combining them simultaneously find out what their resultant observed effect should have been and will be. It has already been pointed out that it is impossible to take account of everything at once, that things must be isolated for investigative purposes one at a time, and, also, that the method of separation and subsequent recombination is essential to the procedure of science. But there is no *a priori* reason to suppose that the effect of two forces or causes, acting simultaneously, can be calculable from the locomotions or effects which they, as components, have *severally*. In modern physics the principle of superposition is found to have less truth than was formerly supposed. Although either of two measured component forces, when considered alone, may express something that is

operationally definite, the combination into a resultant of two of them, both having been measured independently and in advance, may yet have no definable meaning in terms of experience. In classical mechanics it so happens that the principle of superposition holds, but it does not in modern physics. There is no reason for taking it for granted, as Lewin does, in psychological field theory (**5**, p. 104; **29**, p. 97; **31**, p. 69; **18**, p. 170; **19**, p. 71).

Psychological Field Dynamism: Dynamical principles in physics prescribe in detail not only the behavior of a system at every instant of time, but also the precise relations between all parts of the system at any given time. Lewin attempts to formulate for his psychological life-spaces principles which will do for psychology what their prototypes do in classical physics. Accordingly, he puts all parts of a life-space into mutual inter-relation with one another. Therefore, whenever some parts appear to resist relationship or cohesion, he must postulate a dynamic wall or boundary between them to account for the greater or lesser degree of abscission obtaining, because dynamical principles forbid the strict isolation of the effects of events from other events. It would be unreasonable on dynamical principles to suppose that of a number of atoms in close spatial proximity each could have an isolated fate, but this is exactly what can happen and does in quantum theory (**8**, p. 300; **22**, p. 190; **27**, p. 71; **15**, pp. 59, 62, 189; **18**, p. 172).

Attempts to retain dynamical principles were given up in physics because they were barren of results and led only to a disagreeable, incoherent complexity of explanation. This was clearly demonstrated when some time ago, following a line similar to Lewin's, a proposal was made that every atom, which was to suffer its isolated fate, be surrounded by a very high potential wall that would screen from it the influences of the other atoms. This postulate made no headway and had to be given up. Only the static fields of relativity and the statistical laws of quantum theory, embodying the so-called 'Aristotelian' concepts that Lewin abhors, satisfy modern theoretical demands (**18**, pp. 172–173; **22**, pp. 191–192).

Retrospective Inference: In the progressive differentiations to which Lewin subjects his life-spaces a marked use is made of retrospective inference, a practice which assigns to them causal characteristics that *by definition* are made to produce at a future date the very manifestations from which these life-spaces were inferred. Laws must have predictive value to be of any consequence. If laws contain retrospective characters, they cannot fulfill their predictive functions in practice, because characteristics that do not manifest themselves until the moment they cease to exist cannot be used for prediction except by those who prophesy after the event. Lewin by the inclusion of retrospective differentiations arrives at 'topological' representations, or, better, diagrammatic formulas which are as complete as Lewin can possibly make them at their present state of approximation. But to include such differentiations in a formula is not the same thing as to *know* them in any given momentary psychological situation to which the formula is applied, for they may represent knowledge which in that given momentary cross-section of the psychological situation is essentially unknowable. Lewin's diagrams, if employed to deduce the course of psychological events, really use the future to predict the future (**5**, pp. 94, 96; **16**, p. 29; **18**, p. 170).

The 'Aristotelian' and 'Galilean' Categories: Lewin makes much of 'Aristotelian' and 'Galilean' concepts which are put into essential opposition to each

other. The division of the concepts of physics into these two categories where we find law versus statistics, entity versus construct, etc., is arbitrary, confusing, and misleading, because the present situation in physics in no way suggests a sharp antithesis between the two views, but rather implies that they are essentially complementary. Actually, physics at present is progressing without a fanatical adherence to either set of concepts. Quantum theory is assuredly 'Aristotelian' in that it is statistical in character; yet it is also 'Galilean' in that it deals with constructs and not entities. Relativity theory, despite the a-dynamic character of its fields, may be classed as 'Galilean' in that it is to a degree lawful in Lewin's sense, though there is a certain independence from observational bases that impart to it an Aristotelian flavor. Still, no contradiction results, for universal law which Lewin extols as 'Galilean' becomes in modern physics merely a limiting case of probability—a concept which Lewin condemns as 'Aristotelian[15] (18, p. 10; 5, p. 77; 6, p. 29; 7, p. 3; 22, p. 287).

Regularities in probability distributions are sought after in quantum physics. Even the individual and rare cases, which Lewin says Aristotle would unjustly term as unlawful and of no interest to science, are included since, falling under the aegis of probability, they are not ruled out simply because they are unusual. The concepts of law and of probability, the latter a statistical conception defined as *a frequency obtaining in a given class of events,* are not the contradictory and mutually exclusive concepts that Lewin would indicate. As a matter of fact, the dichotomy which Lewin sets up between 'Aristotelian' and 'Galilean' concepts falsifies the whole picture of theoretical science and gives an erroneous idea of what is going on in present-day physics (6, pp. 50, 63, 91, 95; 27, pp. 23, 52–53).

The importance of Galileo to modern science rests mainly in his recognition of the place of experiment in science—a recognition which laid the foundations of a new methodology. Experiment was to be the sole arbiter of facts; hypotheses were to be modified accordingly. Concepts related to this new order of things can be aptly termed Galilean. Yet, physics, having derived much of its strength from Galilean concepts, has reached the point where its recent advances have again given it a decidedly Aristotelian cast. Eddington states, for instance, that the book of nature need hardly be consulted at all for its theoretical formulations, and observational tests now provide the same kind of perfunctory verification as they would when applied to geometrical theorems. Theories have become purely deductive, being based on epistemological principles and not on 'physical' hypotheses; while all that is now required of observation is evidence of identification between the mathematical symbols employed and the entities of the experimental physicist. In other words, there is nothing in the whole system of the laws of physics that could not be deduced unambiguously from epistemological considerations alone without any recourse to physical hypotheses as such! This is from a certain standpoint unadulterated Aristotelianism, and it must be emphasized once again that developments in physics such as the above can-

[15] This generalization of physics is known as Bohr's correspondence principle, which states that all the deterministic classical laws of mechanics and electromagnetism, including the modifications introduced by relativity theory, are simply the limiting form assumed by the indeterministic statistical formulas of quantum theory when the number of quanta or particles concerned is very large. The molar laws of the former are merely a simplified form to which the statistical laws of quantum theory converge.

not be at all adequately described or even indicated in terms of Lewin's 'Aristotelian' and 'Galilean' concepts which are simply here not *à propos* (6, pp. 18–19, 43–47, 56–57, 62, 78, 82–85; 7, pp. 3–5, 327; 31, pp. 20–21).

One of the greatest accomplishments of Galileo consists in his liberation of physics from Aristotle's practice of misapplying auxiliary concepts, correctly usable in a limited domain of experience, to regions outside their area of general validity; for it was Galileo who took the basic concepts of Aristotelian physics like *light* and *heavy, above* and *below, natural* and *forced* motion and showed that it was the use of these restricted concepts outside their realm of validity that prevented a proper understanding of physical phenomena. It is, likewise, the contention of this paper that Lewin is guilty of conceptual misapplication—the very practice for which Galileo condemned Aristotle. This has occurred again and again in Lewin's handling of concepts appropriate only to physics and mathematics, and it will be specifically shown in the next sections how this practice is continued in his treatment of the mathematical concepts of topology as they have been made to relate to psychology (3, pp. 37–38; 10, pp. 45–46).

IX. TOPOLOGY

It is generally agreed that the theoretical development of any science through the use of mathematics imparts to it an internal coherence and cogency that otherwise would be lacking; and it is on topology that Lewin professedly draws to set up for psychology an embracive mathematical framework in order to accomplish through mathematical intervention in this field what has in like manner been so elegantly consummated in physics (18, pp. 78–79). But is it the topology of the mathematician that is manipulated into the theory, or has Lewin employed an individual variety of topology which has so few points of resemblance to the original that it is not even mathematical, let alone topological? The following discussion will attempt to provide the answer.

Boolian Algebra: Topology concerns itself with the properties of sets of points. A given set is not to be thought of as merely a collection of enumerable points, but as something which is determined by one or more properties that test the claim of any point to be considered a member of that given set. Sets may be added and multiplied, although the signification of these operations is different from that attached to ordinary addition and multiplication, summation being the totality of points in the added sets and multiplication those points which alone are common to the sets involved. The definitions of these two operations in topology are practically the same as those given by Lewin. However, although these operations have all the formal properties of ordinary addition and multiplication —a fact of great importance—Lewin nowhere makes any essential use of them, a characteristic indication, as will be seen, of the fragmentary and circumscribed use to which topology is put by Lewin (16, pp. 30–31; 18, p. 87; 24, pp. 3–4).

A remarkable property of duality exists wherein a properly expressed formula remains true provided all the symbols of certain pairs of operations, such as those for addition and multiplication, are interchanged; so that, as an instance, the distributive law, $A \cdot (B + C) = (A \cdot B) + (A \cdot C)$, becomes by the application of the principle of duality another true formula, $A + (B \cdot C) = (A + B) \cdot (A + C)$. This is a remarkable property; as remarkable as the principle of duality in projective geometry where, for example, a properly ex-

pressed theorem concerning a situation in a space of three dimensions remains valid if the words *point* and *plane* are everywhere interchanged. Yet one can peruse Lewin's works with the greatest diligence and not find a hint of this basic principle anywhere despite its obvious importance for the derivation of new formulæ (24, pp. 4–7, 9; 33, pp. 28–29).

The algebra of sets as indicated so far is nothing more than the Boolian algebra of symbolic logic—the science of symbols that denotes logical properties and their combination according to rules which correspond to the laws of logic. It is interesting to note in this connection that an examination of the Eulerian diagrams or circles of formal logic shows that more Boolian algebra inheres in them than in the topology of 'topological' psychology. Nevertheless, no one has gotten around to speaking of a topological logic and for one very good reason—Boolian algebra is not yet topology. It has a long way to go, as will be seen. Incidentally, the arguments concerning the use of Euler's circles in logic are very reminiscent of Lewin's arguments in behalf of his topological figures; but it should be noted, in contradistinction to Lewin's position, that, throughout, the use of Eulerian diagrams is admittedly and frankly schematic and nothing more, in the same way as are the countless configuration diagrams of physics (13, pp. 156–159, 162; 16, pp. 14, 30; 18, pp. 8, 16, 78–79, 107–108, 110; 22, p. 129).

The Five Fundamental Concepts: What then is topology? This question cannot be answered immediately, for as it is sometimes said (24, pp. 45, 206–207), there are five concepts that are fundamental to it, and it is with properties expressible in terms of these concepts that topology is concerned. These concepts are those of *derived set, closure, open set, closed set,* and *convergent sequence.*

It is not the purpose of this paper to define in precise symbolism the meaning of these five fundamental concepts,[16] but to state categorically that they do not exist in Lewin's formulations. The slightest acquaintance with any standard work on topology should demonstrate this conclusively, and without an effective incorporation of those five concepts topology simply does not exist. None of these five basic concepts are explicitly incorporated into the topology of 'topological' psychology, though there is an implicit suggestion of closed and open sets in Lewin's closed and open regions mixed up with obscure boundary notions. However, even closed and open regions are dismissed as matters of minor importance. Furthermore, in Lewin's terminology a set is made equivalent to a region and a point to a part-region, that is, a subset, in spite of the fact that a point can be only the ultimate element of a set. This sort of confusion and error is typical of the kind everywhere present in Lewin's mathematical treatment (16, pp. 30–31; 18, pp. 89, 94, 105; 24, 19–24, 28–29, 32).

Abstract Topological Spaces and Deductions: Any abstract topological structure may be set up in a set of points provided rules are given which settle definitely the meanings of the five fundamental concepts: *derived set, closure, closed set, open set* and *convergent sequence.* Lewin does nothing in any way suggestive of the above procedure in setting up the conceptualized life-spaces of 'topological' psychology. As a matter of fact, abstract topological

[16] Professor S. Lefscheftz, who has read this paper, feels that there are not five fundamental concepts but only one, open set, the others being definable in terms of it. This criticism seems to be one point of view among mathematicians and is not vital to the position taken in this paper.

spaces, where all the five fundamental concepts are taken as independent primitive ideas, are too general to lead to any recognizable results, as their theory consists of little but *definitions*. To make any progress with the mathematical theory some additional restrictions—that is to say, axioms—besides those implied by the definitions must be imposed (24, pp. 48, 206–207).

The topological definitions which Lewin employs and renders meaningless through an unwarranted recasting to fit preconceived molds would on another account be sterile and devoid of power, for these definitions by themselves, torn from the body of the enmeshing mathematical theory, are unproductive of deducible conclusions. The host of theorems that form the actual machinery of topology should have been made to function and so to take over the work of rigorous deduction. Lewin in reality does not utilize *one single theorem* of topology. Always there is an interminable use of a few *definitions* ripped out of their proper context. This being the circumstance, it is futile to insist that any closely articulated system has been achieved. Newton, for example, in evolving his system of mechanics did not employ for the purpose a few isolated definitions from the calculus. He used the *body* and not just some of the roots of that particular mathematical theory, so that by the whole application of the calculus, formula after formula methodically arises (16, pp. 30–31; 26, p. 64).

Topology and the Part-Whole Relation: Lewin like all Gestaltists has emphasized the part-whole relationship, but to the point that he sees it a veritable foundation stone of topology. He states, "From the point of view of psychology it is especially interesting that one can use the part-whole relationship as the basic one. This basic theorem states that for any two objects, U and V, of a system for which certain conditions hold, the following relationship shall or shall not be valid: U is a part of V (this is equivalent to 'V includes U')" (18, p. 53). Now, except for an identity of nomenclature, what does the part-whole relationship of psychology have to do with the part-whole relationship of topology? As a matter of fact, the standard method of founding topological theory is to assume that any one of the five fundamental concepts has in some way been given a meaning and then to define the others in terms of it. Any one of the five fundamental concepts can be and, in fact, has been made the starting point of an axiomatic theory in this way. Riesz started out with the derived set, Kuratowski with closure, Sierpinski with closed set, Alexandroff with open set, and Frechet with convergent sequence. The part-whole relationship that Lewin emphasizes through so many pages exists in many forms throughout all of mathematics: the calculus, the various geometries, etc. It is not a peculiar possession of topology (24, pp. 206–207; 27, p. 109; 18, pp. 53–54, 171).

What is Topology? By topological equivalence of sets it shall be understood that sets associated with different metrics [17] shall possess certain identical properties despite the varying metrics involved, in which case these metrics are said to be topologically equivalent. The geometrical meaning boils down to this —that a change of metric represents a bending and stretching of space without tearing so that topology may then be roughly defined geometrically as the study of those properties of point sets (geometric figures) which remain invariant when subjected to bending and stretching or as the study of those properties that figures, whose size and shape

[17] Metrics are employed as auxiliary concepts in topology contrary to Lewin's belief (19, p. 2; 23, p. 19–20).

have been ignored, have in common (14, p. 2; 24, pp. 45–46, 48–49).

By introducing the concept of topological equivalence under that of homeomorphism, it is possible more precisely to define the one fundamental problem of topology: given two geometrical figures, when are they homeomorphic? Two abstract topological spaces are said to be homeomorphic, if they have the same topological structure; that is to say, if there exists a one-to-one correspondence between their points correlating all sets of the one, as expressed in terms of the five fundamental concepts, with those of the other. For example Fig. 3 shows that the real Euclidean line is homeomorphic to the

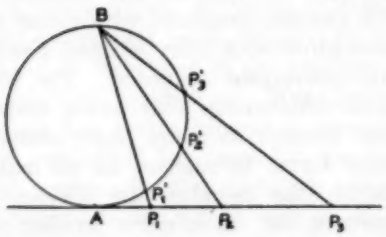

FIG. 3

punctured circle, that is to say, to the circumference of a circle from which one point has been removed. To see this, take a circle tangent to the Euclidean line at *A* and let *B* be the diametral point of *A*. Puncture the circle by removing *B*. Then, with *B* as center, the points of the punctured circle can be progressively projected in a one-to-one bicontinuous way into the points of the Euclidean line. If the real Euclidean line is closed by inserting the point at infinity, the real projective line is obtained. This is homeomorphic to an ordinary circumference, for then the point at infinity corresponds to the point *B*. All this sounds very strange, if one's acquaintance with topology has been derived only through a study of Lewin's works, and is adduced here precisely because there is no better way of demonstrating the complete difference between the topology of mathematics and that of Lewin (14, pp. 2–4; 24, pp. 51, 54–56, 65).

Coördination of 'Topological' Psychology to Topology: Lewin mentions the topological invariant property of connectedness of continua on various occasions, but the requisite mathematical precision again is lacking. In topology notions of connectivity and continuity, between which a very close relation exists, imply an infinitely structured space (that is to say, an infinite set of points)—a fact which Lewin acknowledges is presupposed by topology. Yet he goes on to state in basic contradiction that psychological regions cannot be divided into smaller parts ad infinitum and that consequently psychological space is finitely structured (3, p. 81; 14, p. 25; 18, pp. 54, 89, 163–164, 168, 173; 24, pp. 36, 62).

Although a point is merely a constituent member of a set of points characterized by certain given properties, according to Lewin a point is equivalent to or synonymous with a set, a psychological region which is supposed, moreover, incapable of further subdivision or structuring into parts! But the promulgation of this fundamental divergency from the topology of mathematics which admits of an infinite divisibility at once vitiates the possibility of applying topology to psychological life-spaces, *for the properties of topology cannot then have established with those of 'topological' psychology those necessary formal mathematical identities that must be recognized between the two before any rigorous mathematical application or coordination is possible*[18] (4, pp. 146, 222; 6,

[18] Lewin denies this. He states that "to apply a system of mathematical concepts in an empirical field one does not necessarily have to prove directly the adequacy of the basic

pp. 81, 173; 10, pp. 80, 99; 18, p. 112; 22, pp. 5–12).

Lewin goes on further to say that, as far as he knows, mathematics has not yet followed up Riemann's suggestion that it is not necessary logically that spaces should be infinitely structured. But finite spaces and geometries have been developed and investigated for some years. In their exposition of a synthesis of projective geometry Veblen and Young make no initial assumptions which determine whether the number of points in a given line (that is, the number of members of a certain set) is finite or infinite, and any number of problems which openly emphasize the finite structure of the spaces involved may be cited from their work. If it were thought worth one's while, it would be with such finitely structured spaces that a geometrical or spatialized coördination of psychology might be attempted (18, pp. 164–165; 33, pp. 25–Ex. 3, 26–Ex. 5, 33–Ex. 2).

Before any utilization of the calculus, Newtonian physics meticulously sets up among other things the necessary one-to-one correspondences; first, between the dimensionless point of space and the idealized point-particle of matter, and second, between instantaneous velocity and the derivative x', so that with the assumption of the continuous nature of the phenomena involved, an application of the calculus becomes finally possible. Even when points are correlated to bodies of large dimensions as in actual practice, it is only after it has been proved permissible to do so (compare with Lewin's ineffectual coördination of a point with a part-region); in other words, masses of matter must first be proven capable of mathematical concentration into their respective centers of gravity with the preservation of all significant properties by the strict correlation of all spatial points with their corresponding point-elements of matter (25, pp. 55–56, 78, 88; 26, pp. 109–110, 131; 16, pp. 30–31; 22, pp. 80–81, 83–84).

It must be concluded from the considerations above that the coördinations between topology and psychological life-spaces have failed of their purpose, being not only imperfect, but in point of fact quite impossible; that in any case, Lewin's topology is not the topology of mathematics.

Sterility of 'Topological' Psychology: Even if 'topological' representations in psychology could somehow or other be justified, their utility to the science is open to real question. If the same 'topological' procedures as practiced by Lewin were applied to chemistry (for example, as when he characterizes chewing and swallowing as regions), how much progress could it have ever made? What strides could have taken place if, at a former stage of its development comparable to that which present-day psychology has attained, chemistry had contented itself with field formulations paralleling those of Lewin's—if, for example, the behavior of inanimate objects in a field threatening combustion had been conceptualized in the same manner as the behavior of human beings subject to the influence of fields of psychological forces (18, p. 97)?

Chemists would then have proceeded as follows: *Problem:* The conceptual representation of setting fire to a piece of cardboard whose kindling point is comparatively high. *Procedure:* In Fig. 4 let A, B, F, G, and H be regions representing activities of various kinds; D a major barrier to be overcome; C and E force vectors oppositely directed.

mathematical axioms of this system one by one. It is as well to prove the fruitfulness of some of the derived propositions of this mathematical system for the representation of the empirical properties of the field in question" (16, pp. 30–31; 23, pp. 111, 117; 9, pp. 214–215).

Then A might represent the flaring up of a struck match; B the application of the flaming match to the cardboard; C the natural tendency of the flame to elevate the temperature of the material in the direction of eventual combustion; D the kindling point—the temperature at which the cardboard will burst into spontaneous flame and which must first

FIG. 4

be reached and overcome; E the resistance of the cardboard to the rise of temperature to the kindling point; F the chemical activity pre-scientifically identified with the process of combustion: the enclosed subregions conceptualizing the generation of heat, the curling up of the burning material, the changes in color, etc.; G the consummation of the series of activities in the direction of the inevitable ashes; and H the cooling residue!

Can anyone claim that by following such a procedure any real knowledge or insight is thereby gained? Is not all this merely a spatial restatement of what is already known verbally? Has anything more been accomplished by this translation of previously recognized facts into the unfamiliar and complicated language of a pseudo-mathematics? It cannot be denied from our point of vantage in retrospect that any advancement of a chemistry employing such methods should appear incredible and hopelessly thwarted at the very start. A parallel conclusion is definitely indicated, namely, that *a mathematically unified field-theory for all of psychology along Lewin's pattern is without significant value and, furthermore, in view of the sharply divergent stand of modern physics, generally impossible* (5, pp. 41, 48–49; 8, pp. 19, 131, 256). The claim of 'topological' psychology to deductive rigor is mathematically spurious and results from an unfortunate misapplication of a very generalized branch of mathematics totally unrelated to the needs of psychological theory.

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PSYCHOLOGICAL MEASUREMENT: NORMATIVE, IPSATIVE, INTERACTIVE

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I. The Need for Integration of Concepts

So long as mental measurement remained largely confined to a few narrow, specialized sectors of behavior, with negligible interaction, certain basic theoretical issues concerning the nature of mental measurement were not provoked by any practical problem, nor was the clarification of these issues necessary to the success of applied psychology. But with the advance of studies boldly directed to measuring the most diverse manifestations of personality we are faced with the necessity either of grasping these theoretical concepts, and stabilizing them in a systematic terminology, or of drifting into a considerable muddle.

The purpose of this article is to discover what is implicit in several recent developments of psychometry and to attack some unsolved problems regarding the units of mental measurement. It aims to show that some hitherto disparate regions of endeavor and discussion—notably those concerning common and unique personality traits (1, 7), concerning Q and R techniques in factor analysis (2, 9, 10, 13), concerning attitude and interest measurement (4), and concerning the measurement of animal motivation (14, 15)—actually hinge on the same concepts.

II. 'Solipsistic' and 'Cosmic' Measurement of Interest

The points at issue are best introduced by a concrete problem. Some ten years ago, when introducing two new devices for the measurement of interest (4), the writer found results not comparable with those obtained by the usual introspective 'interest measurement' in which the subject responds to a questionnaire. For lack of more precise terms he suggested at that time that two varieties of interest units be considered, and named respectively 'solipsistic' and 'cosmic' units.

The difference may be illustrated by the problem of having to measure the interests of two men, each of whom reports that one-fifth of the total circle of his present interests is taken up by gardening. This score of one-fifth can be called a solipsistic measurement, because it has no comparability or reference to other units than those within the life of one man. It cannot be confirmed or denied by any other kind of measurement. On the other hand (if for the sake of discussion we equate gardening with planting potatoes) we can measure the number of rows of potatoes which each man digs, whereupon we may find that though gardening represents a fifth of the total life interests of each man, the second man digs twice as many rows as the first.

The term 'cosmic' was suggested for this measurement of the *effects*, social, biological and physical, of a behavior trait. The breadth of the expression may perhaps be excused because of its service in reminding one how widely ramifying are the effects which need to be taken into account in such measurement; but the term *'interactive'* or *'field'* measurement is now suggested instead, for one is measuring the interaction of the individual and his environment, or the effect of the trait upon the field. The great virtue of in-

teractive measurement is that it promises to tie up relatively directly with the measurements of sociology, economics and the physical sciences. It is measurement in terms of the actual physical and biological *effects* of behavior, usually, in test situations, within a restricted framework defined by the test. It recognizes the oneness of the organism-environment and pays tribute to the oft-forgotten fact that a trait is never resident only in the organism but is a relation between organism and environment. Interactive measurement promises direct calculations between personality trait measurements and happenings in the external world and seems, however dimly, to open up the possibility of ultimately reducing psychological measurements to those basic space-mass-time units common to the physical sciences and perhaps ultimately to all sciences.

The variety of measurement with which most psychologists are busied, is, curiously enough, neither of the above two basic forms but a derivative of the second. In virtue of their actual, interactive performances—usually expressed as the raw score obtained in the situation of *interaction with a very particular and artificial test situation*—the subjects are placed in order relative to one another and assigned a standard score in terms of the population distribution, as happens, for example, in an IQ score or a percentile score on the College Entrance Examination. Terms such as 'populometric' or 'parametric' might aptly be used to describe this measurement, indicating the relativity to a population or population parameter, but the word 'normative' (although having the slight disadvantage of suggesting an authoritative as well as a merely popular norm) has become well established in precisely this sense and will accordingly be thus used in this discussion.

To a first survey of the measurement field there are, therefore, three recognizable kinds of measurement. First, solipsistic measurement, based on introspection and therefore relative to the individual's universe as perceived by the individual. It is exemplified in current practice by many interest, attitude, value and personality questionnaires, though, as we shall discuss shortly, these also attempt to give a normative character to the final measurement. It has occurred earlier in the history of psychology in many frankly introspective studies on, for example, feeling quality and quantity, and in the psychophysical studies of sensation, notably of vision and hearing (12, p. 117). Interactive or field measurement is illustrated in earlier psychology by reaction times, by nonsense syllable and maze learning studies, by psychogalvanic reflex and other near-physiological measures, and more recently by animal studies (*e.g.*, measures of 'emotionality'), measure of work, fatigue and fitness (*e.g.*, the Schneider index) by raw scores on educational achievement tests and specific performances in industry. Normative measurements need no illustration, having become the very type of psychological measurement.

III. Theoretically Possible Forms of Measurement

Further consideration will reveal, however, that besides these three forms there are several other conceivable forms of measurement implicit in the combinations of the characters of these. It happens merely that the first three have become conspicuous through the frequency and fashionableness of their use.

In the first place, measurements using the individual as standard, analogous to the precise sense in which the group is used as a standard in normative measurements, can be derived from interactive as well as introspective, solipsistic observations. Again, rightly or

wrongly, psychologists have attempted to build up a normative kind of measurement out of solipsistic data, in questionnaires, etc. In fact the situation is that we have two kinds of raw 'measurement,' introspective (solipsistic) and behavioral (interactive), and basically three ways in which the raw measurement may be evaluated. It may be left untransformed, or made into a derived measurement expressed relative to standards in the self, or into a derived measurement expressed relative to standards in the group. As will be seen later, there are also derivatives possible from these, *e.g.*, measures relative to the self may secondarily be used as a basis for measures relative to the group.

The essential relationships are indicated by the diagram below, in which the divisions from left to right are made according to the character of the raw observations and the vertical divisions according to the reference systems with respect to which the units are finally scaled. The term *ipsative* (Latin *ipse* = he, himself) is suggested as a convenient one for designating scale units relative to other measurements on the person himself; so that the varieties of behavioral measurement become three: *ipsative, normative* and *interactive.*

Whether, in any one of these sections, the measurement is literally quantitative or merely a ranking in a continuum which can be quantitatively regarded, is basically unimportant. But there must be a conceivable continuum, having a single quality or direction, within which measurement lies. Normative and ipsative measurements have their nature defined by an interactive continuum, *e.g.*, IQ's are in a continuum of mental capacity. Similarly a solipsistic measurement, *e.g.*, of interests, active continuum, *e.g.*, IQ's are in a continuum of mental capacity. Similarly a solipsistic measurement, *e.g.*, of interests, must lie in a continuum of interests. One cannot mix interests, abilities etc. in a solipsistic continuum, unless there is some character in common to these diverse introspectible experiences which permits their being assessed one against another or against some total for the person of which they all form a part.

SYSTEMATICS OF MEASUREMENT

Nature of Reference System	*Nature of Raw Data*	
	Behavioral (Dimensions of external world.)	***Introspective*** (Dimensions of consciousness.)
In terms of literal, logical dimensions defining events.	***Interactive*** or Absolute.	False Interactive or False Absolute. (Impossible in the absence of E.S.P.)
In terms of a population of measurements provided by a population of persons.	***Normative*** 1. Simple Normative. 2. Ratio Normative. 3. Ipsative Normative. Also Spurious Interactive Normative.	False Normative.
In terms of a population of measurements within the individual.	***Ipsative*** 1. Simple Ipsative. 2. Ratio Ipsative. 3. Fractional Ipsative. 4. Normative Ipsative.	***Solipsistic*** 1. Ratio Solipsistic. 2. Fractional Solipsistic.

Considering for a moment the behavorial data measurements alone, one observes that several derivatives are theoretically possible from the three primary forms, and that most of these find representation in some branch of practice. Thus ipsative measurements may be expressed in standard scores either from a population of measurements, or as ratios of a single measurement (*e.g.*, Vincent curves in learning) or again as 'fractional' ipsative measurements in which the units are fractions of some total possessed by the individual (*e.g.*, interest measured as a disposition of the individual's total time or other objective measurement of interest). Finally there are 'normative ipsative' units, in which interactive measurements which have first been made normative (relative to the group of persons) are themselves used as a basis for ipsative re-calculation. The value of this last procedure is discussed in the next section.

Normative measurements have parallel derivatives to these. In addition to the simple form there are ratio and ipsative forms. In the former the score is expressed relative to some single, but group-derived standard, *e.g.*, that of the average or modal man, or some authoritative standard. Examples are rare: the yard was such a unit when it was the length of the king's arm. Some vocational selection test units are in this form (fractions of the output of the average man) and sometimes individuals bave been scored against the performances of 'median samples' (5, p. 79). In the latter form ipsative normative scores are interactive scores first scored ipsatively and later normatively from the ipsative units. This does not produce the same units as carrying out these procedures in the reverse order. It has the advantage of permitting the normative scoring of responses which could not be readily scored directly by means of rankings by a set of individuals of a set of impressions. The category of spurious interactive normative is discussed in the following section.

Regarding the whole division of introspectively based scales, it is the contention of the writer—but one with which some outstanding psycho-physics researchers disagree (11, 12)—that in the present state of our understanding of consciousness no absolute or normative, but only solipsistic measurements can be derived from introspection. The issue is developed in Section V. For the present we shall consider that the inter-individual comparison of introspections offers merely approximate measures, beset by unknown chance and systematic errors but which may act as a rough scaffolding for the planning of more behavioristic measurements.

IV. Relationship to the Q and R Techniques of Analyzing Co-Variation

Not long ago a dispute arose between Burt (3) and Stephenson (10) as to whether Q technique—the correlation of persons instead of tests—could yield correlation clusters or factors extra to those found by the usual R technique. Stephenson correlated persons when the measurements were expressed (*a*) as raw scores, (*b*) as standard scores, and (*c*) as self estimates, *e.g.*, of the order of 'importance' of a series of traits within the personality or of the attractiveness of a series of pictures (9, 10). He also proposed to use populations of attributes for one person only and to set out the results (10, p. 287) of a "factor analysis performed upon myself alone as variable."

Stephenson argued, perhaps with the pioneer's lack of caution, that ordinary R technique findings and Q technique results on the (*a*) basis (above) are "by very definition statistically independent of one another in general" and stated

that the former can "not be regarded as the direct obverse or mere transpose" of the latter. Expression of results on the (*a*) basis, he points out, is the logical conclusion, in mathematical terms, of Gestalt theory as applied to personality research.

Burt (3), on the other hand, considered that Q technique on this interactive, raw score basis is 'meaningless' and Thomson (13) dismissed it as 'absurd,' because therein one is compelled to take the average of a collection of test scores (those of one individual) expressed in diverse modalities and units. If, on the other hand, the (*b*) basis is followed, and the scores are standardized so that the matrix of scores and persons becomes 'doubly centred,' the very same factors, Burt argues, will appear alike from Q and R techniques, but in the former case they will have loadings in persons instead of tests. He concludes that the choice between these two techniques has to be made, therefore, simply on the basis that "where we have more tests . . . than persons [we] start by correlating persons, and, where we have more persons than tests [we] start by correlating tests." Thomson (13) agrees that Burt's 'reciprocity principle' between R and Q technique factors is accurately true in some circumstances and approximately in others. Both Burt and Thomson have used this form of Q technique ([*b*] basis) to assign weights to the marks of different examiners grading material for which there are no objective standards of validity.

It is scarcely to be expected that the above aspects of factor analytic systems can become clear in such brief paragraphs to readers unfamiliar with the literature in the field, but it is not practicable to enlarge, since our concern here is not with factor analytic systems as such but rather with the assumptions regarding measurement made by these systems. We find, first, in Stephenson's (*b*) treatment a measure normative with respect to the group which is later re-scored ipsatively. In the (*a*) treatment there is a bastard ipsative measure, formed by putting together interactive measurements of quite different modality. In the (*c*) method we find supposedly normative measurements made by treating solipsistic measures as if they were comparable in the sense of being in a single universe.

To the first of these no objection can be raised. Both the normative treatment of scores primarily ipsative (and fundamentally interactive) and the ipsative re-scoring of measures first put in normative terms seem legitimate and real. The numerical results of these processes on the same raw scores, tests and populations, as pointed out above, are not identical and the measurements obtained cannot be considered purely ipsative or purely normative. They are measures relative both to the group and the individual, but they give greater weight to whichever of these relativising processes has been carried out last.

To the second the objection can be raised which Burt and Thomson have already entered, namely, that there is no continuum in which diverse interactive measures can be simultaneously expressed, or averaged, either in the cosmos or in the individual. This same objection has to be raised also against Stephenson's third (*c*) basis, at least as it is sometimes used, for the attempt is made therein to compare different introspective qualities. But in much of this latter measurement, principally as it is employed and maintained in questionnaires, the further objection can also be raised that even if such continua existed there is no evidence, since they exist in the solipsistic universes of different individuals, that they are the same.

V. THE LIMITATIONS OF SOLIPSISTIC CONTINUA

At this point it is necessary to scrutinize more closely the implications of existing mathematical treatment of solipsistic data. Such factor analysts as have used this data, *e.g.*, Stephenson and Burt, have cautiously assumed only that the subject can rank his introspected experiences. They have asked him, for example, to place in order his attractions to fifty different vases in the British Museum, or to place twenty personality traits in order of 'significance' with respect to his own personality. The psychophysicists, even when they have not conceived difference thresholds in entirely behavioristic operations, have still shown considerable caution and have only occasionally assumed the ability of the subject to judge quantitative differences in sensation, *e.g.*, in asking for colors twice as saturated or tones twice as voluminous (12). But the makers of questionnaires have quite frankly built upon quantitative solipsistic estimates, sometimes overtly and a great deal by implication. Personality questionnaires ask the subject whether he is moderately or greatly afraid of high places or whether his embarrassment in certain situations is above or below average (presumably not only his own average, but even that of others!).[1]

Now, whether measurement is in terms of behavior or of consciousness, the mathematical psychologist has a right to demand, as a condition of measurement in any reasonable use of the word, (*a*) that the measurements shall lie in a single continuum, of constant and definable direction, and (*b*) that this continuum shall be the same for all subjects. There are, also, further conditions within (*a*) necessary for the use of true cardinal numbers in measurement, as Johnson (8) and Stevens (11) have clearly demonstrated to psychologists, but these are not in question at the moment. The two criteria above, as consideration of the field of mental measurement in general and of the specific examples below will show, have not been satisfied in much alleged interactive and normative measurement. But it is *the* standard objection to introspective 'measurement' that either or both of these criteria are *never* satisfied therein.

Returning to Stephenson's (*c*) or self-reported variety of Q technique, we see that in fact it does not make the second of the above assumptions. When subjects rate vases according to aesthetic appeal it is not assumed that this continuum is the same for all; indeed, the object of the correlation study is to discover how the divers directions of these continua in different persons are related. (Incidentally, one may note that in 'correlating persons' one is still actually correlating test measurements —or pseudo measurements—, but in continua which lie in the individual instead of outside.) The objection to basing the Q technique on solipsistic data lies in the failure to meet the first criterion, namely, in the assumption that any single continuum could exist for some of the very disparate kinds of variables which are thrown together. When subjects are asked to rank twenty diverse personality traits (abilities, sentiments, temperamental tendencies) with respect to 'importance for one's personality' one may well doubt whether such a dimension exists, or whether the subjects have any means of rating it if it does. On the other hand, when a set of behaviorally defined variables are used, as in the (*b*) form of Q technique, (measurement in standard scores), the dimension or continuum is, for each per-

[1] By implication they also require measurement in such questions as: "Do you prefer to work alone rather than with others?", "Do you usually feel well and strong?"

son, one of 'extremeness' or 'eccentricity.' If all the variables are of one kind, moreover, the dimension is even more clearly defined, being, in the case of interests, 'eccentricity of interest.'

Personality questionnaires, some attitude scales and all subjective type interest tests, on the other hand, blithely make both assumptions, and their eager promoters seem ready to believe that high consistency coefficients remove the measurements from the realm of solipsism to that of interaction—as if delusions could not be consistently maintained! For this reason we have labelled, in Diagram I, the statistical treatment of introspective data 'false normative' measurement. Philosophically, false normative and solipsistic varieties of measurement should perhaps not be denied the title of mensuration; for the units and the results of manipulating the units may be as interpretable as the results of carrying out processes in some formal system of non-Euclidean geometry, in which certain definite but cosmically untrue assumptions are made. It remains, however, an intellectual amusement without relevance to psychometrics.[2] Mathematical processes *can* be carried out on solipsistic measurements, which are real enough, but they must properly be confined to the universe of one mind, which makes their utility extremely limited.

VI. Forms of Measurement Possible for Common and Unique Traits

Recent years have seen sharpened discussion directed to the clarifying of concepts dealing with the systematic description of personality. It has become evident that both common and unique traits have to be measured for the complete definition of any given personality (1, 7). A common trait is a pattern in which all people possess some amount of endowment, whereas a unique trait is entirely peculiar to the individual.

Published views still show some disagreement or confusion as to the nature of unique traits, which we may best attempt to dissolve by considering a physical example. Every man's hand shows a unique pattern of finger lengths. Nevertheless, individual hands can be accurately described either in terms of quantitative variations of the five 'common traits' constituted by the fingers or of certain abstract 'common traits,' constituted by perhaps a couple of mathematical general factors (and maybe specifics) through which all the finger lengths can be defined. Any pattern of finger lengths can be expressed by these quantitative variations in common traits, but sometimes a truly unique trait, such as a six-fingered hand, may appear. The question now arises whether this pattern, like any other unique pattern, can be expressed as a quantitative variation of common traits, or whether the appearance of an entirely new pattern element makes recourse to a unique pattern formula inevitable.

The most obvious and practical procedure is to consider the sixth finger a unique trait and to measure it in interactive units of its own. (It could be measured in units of other finger lengths, but this is purely an accident of our simple example in which all variables lie in the same physical space. A

[2] Nevertheless, the fact that the subject responds to an introspection-demanding question in a certain way, *e.g.*, ranks vases according to the amount of subjective emotion provoked by them, is as much an interactive measurement as an observation of how long he stares at the vase, or how much he will pay for it, providing the response is treated as a *manifest verbal response* and not as a *statement of truth* in itself. Indeed, the relationship between these self estimates and other interactive measures promises to provide a particularly clinically discerning measurement, as recent research indicates.

unique, new mental trait would, by definition, introduce a new dimension not represented in the common traits. But the mathematically sophisticated will object that so soon as a single second instance of a six-fingered man is discovered, the trait ceases to be unique, for a new continuum can be set up and the men can be measured in terms of standard scores, from a common average in a common trait. Unique traits, by their very nature, must be relatively infrequent, at least among the socially important examples of behavior and in other than dynamic trait categories. But the logical mathematician may now show that it is possible to proceed from two instances of the trait element to only one and still obtain a dimension and a possible continuum. For it can be assumed that all people have a sixth digit, of zero magnitude. Between this zero and the single positive quantity a common scale of six-fingeredness can be worked out and correlations can be obtained with lengths on all the other fingers, so that the trait has lost its unique isolation. From this point of view the unique is only the extreme instance of the common; it is the unusual score carried still further out on the tail of the distribution; it is an asymptote of the normal.

Logically, therefore, the unique trait cannot perhaps be defended in its claim to an absolute category. A common dimension can always be set up in which the rest of the population has a score faintly below average and the single individual a score appreciably above average. But this is a barren mathematical and logical victory, for in practice nothing is gained. The common dimension we purpose to use is uninhabited and desert, and the scale units of normative score which we might set up depend for their magnitude and stability on the frail thread of a single individual's score. There is no point in attempting to retain normative scoring for unique traits.

Consequently it is a cardinal difference of common and unique traits that whereas the former are commonly expressed in normative measures the latter are best expressed in interactive measurements. The present rarity of interactive measurements in psychometric practice is a startling confession of the extent to which the 'inconvenient' unique traits have been ignored. For in vocational guidance there are unique skills and rare but powerful interests which need to be given as much respectful attention as the psychotherapist gives in the consulting room to unique emotional experiences. The advance of psychological measurement from the classroom and laboratory into the consulting room demands clear vision of the role of the unique trait and the prosecution of a science of interactive measurement.

VII. THE EXPANDING FUTURE OF INTERACTIVE MEASUREMENT

It is unlikely that the present phase of meager attention to the principles and possibilities of interactive measurement in psychometry will persist long, for in its nature interactive quantification is indeed the queen of measurement. In the first place, true ipsative measurements (*i.e.* measurements within the personality but with respect to a continuum possible for all personalities) and true normative measurements are bound to be founded on interactive measurements. In the second place, the interactive measurement is not only more fundamental but also more permanent, surviving the dissolution of populations or the vagaries of cultural change. For example, it is more valuable to know that Shakespeare at nine had a recognition vocabulary of thirty-eight thousand words than that he stood at the ninety-second percentile in the

County of Warwick Vocabulary Test of that day. A third advantage of the interactive measurement is that it is frequently more immediately meaningful and useful in prediction or in adjusting the individual to a task or situation, notably in vocational selection. To an employer seeking a stenographer from a business school it is less helpful to know that she achieved a B − in the school's standard test than that she can type an ordinary letter without mistakes or take dictation at a rate of 110 words a minute. On account of the lack of interactive measurement scores, few psychologists today, even of great experience, could say what IQ would be necessary in a child of ten in order that he may understand insightfully the principle of, say, logarithms.

Finally—and this is the most important feature of interactive measurement —attention to interactive units forces the psychologist to analyze more profoundly the psychological functions he is measuring. If individuals can be given a score simply from putting them in rank order—and people can be put in rank order for anything under the sun—there is very little incentive to find the exact nature of the thing with respect to which they are being put in rank order. The facility with which IQ or percentile scores can be used in educational and placement problems has apparently obscured interest, for example, in the problem of the nature of intelligence at different age levels (its factor structure in terms of test situation variables, 6), while the readiness with which interests can be ranked solipsistically in questionnaires seems to have made it superfluous to ask 'What is interest?' On the other hand Tolman's attempts to measure interactively 'means-end-distances' (14) are very provocative of thought concerning the nature of drives.

Let us examine more closely the consequences which may be anticipated from a more thorough promotion of research on interactive measurement.

VIII. The Possibility of Unification of Interactive Measurements With Those of the Physical Sciences

The attractive possibility which glimmers in the inherent character of interactive measurement is that it may lead to a reduction of psychological units to those of physics and join the sciences in an illuminating monism. We might, for example, measure the interest of our man in his garden in terms of the physical energy which he applies to this part of his environment, in centimeter-gram-second units. The true interactive measurement might thus tie in at once the problems of psychology with those of physics and also, if a common reduction is possible for these sciences as well, with economics and sociology. But definition of units of biological sciences must always require reference to a pattern of expression of energy over and above the units of energy itself. Further, it seems certain that many psychological traits, by their very nature, can only have their character fully defined through reference to the *total* Gestalt in which they lie—the only true Gestalt which exists, namely, the whole cosmos. Consequently a complete and accurate interactive measurement would need to be in terms far less local in character than those on which we contingently base normative measurements, and even a token interactive measurement would need to have its reference to these wider consequences of behavior clearly understood by the measurer. For example, interest measures might have to be scaled in some complex units which are functions of time and money spent on objects, in relation to physiological conditions and the structure of the social group. Before such an interest unit can

be derived from c.g.s. units the economist, for instance, will need to reduce monetary units into physical energy units in relation to a social structure pattern. In short, the reduction to interactive units in the same universe as those of physical science will only be possible in one socio-biological science when it is possible in others. Most interactive measurements at present are thus of a 'token' variety, not pursuing the full interactive effects of behavior.

The goal of a monistic interactive measurement system is speculative and at present hopelessly out of reach. Possibly, by the very nature of things it can never be reached. It may be objected, for instance, that there is a systematic barrier to putting psychological characters in physical terms, namely, that the results of behavior do not usually show up in the physical world but in the behavior of other organisms. Even if the interaction of individual and society eventually spills over in effects on the physical environment, those effects are usually impracticably remote from the original individual behavior.

But these objections do not close all avenues. For the psychologist can base his measures on physical units in proportion to the biologist's success in expressing life processes, *e.g.*, metabolism and growth, in chemical and physical terms. One might, for instance, measure a speaker's endowment in the trait of eloquence by means of the increase, in grams of oxygen consumed per second, in the metabolism of an audience of one hundred listeners, or measure the dominance traits of a leader by the extra units of work he can get from a group by driving methods.

Closer scrutiny will show that the reduction of interactive measures to common c.g.s. units offers greater difficulty with some traits than others, notably with temperamental and ability traits, which do not themselves have such direct effects[8] upon the environment as do dynamic traits. Yet these examples merely bring out the inescapable conclusion that with all traits the interactive measurement will never be reducible to power or work units alone. Always there must be the additional and qualifying statement about the pattern in which the energy is discharged

IX. The Motley, Deceptive and Segregated Character of Psychometric Data

Any survey of current, practical psychometric procedures will reveal at once (*a*) the great predominance of normative measurement, and the rarity of interactive measurements of a kind suitable for use in their natural form, and (*b*) the tendency of certain forms of measurement to become segregated, as by an uncritical tradition, predominantly in some special branch of psychological endeavor. Thus interactive measures segregate most in animal psychology and psychophysics; ipsative measures in learning; solipsistic measures in interest study and in psychophysics; false normative measures in personality and attitude questionnaires, and true normative measures in education, intelligence testing and guidance work generally.

The most disturbing fact revealed by the inspection of existing practices in the light of this systematization of measurement lies, however, in the finding that many measurements implicitly assumed to be of one kind are actually of another. Most noteworthy among these masqueraders are the many false normative measures in personality, interest and attitude scales, which are basically solipsistic measures unfit for treatment

[8] Abilities and temperament traits show themselves as modifications in the expression of dynamic traits. They are formal characters defined in relation to certain complexities of the environment.

as the true normative measurements they pretend to be. Next come some psycho-physics measurements, *e.g.*, of the loudness function, which are averaged as if they were interactive measurements but which are really solipsistic. Then there are many measurements in learning, forgetting, fatigue, etc., which, because of their context are thought of as interactive measures, and might readily be applied to practical problems as such, but which are really ipsative. For example, if the fatigue of an hour's riveting is measured for each person by dividing the number of rivets fixed in the last ten minutes by the number fixed in the first ten minutes and a mean value of 20 per cent reduction is obtained, it does not follow that a factory group fixing two hundred thousand rivets at the beginning of the hour is most likely to fix only one hundred and sixty thousand in the last fraction of the hour. For the measurement of fatigue for each person is an ipsative measurement which cannot be added directly as can interactive measurements.

Animal psychology has, from the beginning, made the boldest bid for a system of clearly behavioral and non-normative measures, *i.e.*, for interactive measurement, yet it has not succeeded in this aim, partly because of current confusion about the nature of measurement. It is usually assumed, for example, that the strengths of drives are not being merely normatively measured. Warden, Jenkins and Warner (15) list the five standard methods of measuring the strength of drive and point out as a disadvantage of the Resistance Method that "the index of the positive drive cannot be reduced to behavioral terms." However, this objection really applies also to the Choice Method and the Obstruction Method, and only the General Activity Method and the Learning Method are really capable of giving interactive measurements. The misunderstanding is perhaps aided and abetted by the 'field' theory which makes 'barriers' something mystically different from stimuli, and by that perhaps pre-dynamic (and certainly pre-ergic) learning theory the counters of which are 'reward' and 'punishment' acting on a generalized 'drive.' For the shock is neither a reward nor a punishment (in the formal sense): but a stimulus situation calling a new erg into operation—the erg or propensity of escape—whereby the total dynamic situation is recreated. Consequently, both in the Obstruction and the Choice Methods the measurement is an ipsative one, measuring drive against drive within the same animal, along some common continuum of 'urgency' of drive. If for a certain animal one finds the strength of the sex drive low, it may not be interactively low, in comparison with another animal. For the strength, being measured ipsatively, in terms of that animal's fear drive, may indicate only that the fear drive is unusually strong. True normative measures cannot be derived from ipsative measures.

In conclusion one may point out that the value of knowing whether a measurement is ipsative, normative or interactive is that the knowledge often safeguards the experimenters against improper manipulation or interpretation of the measurements. There is actually little danger of measures in these different units being added together, for where the statistician might be tempted there are usually obvious differences in modality or origin, but other manipulations and interpretations are more frequently at stake. It would probably be advantageous to psychometrics to have every measurement simultaneously expressed in the three possible parallel forms, always remembering, incidentally, that the interactive measurement on which the ipsative score is based may be different (as in the above ani-

mal study example) from that on which the normative measurement is based. And though ipsative and normative measurements will generally have a greater appeal to the psychometrist, being chosen generally to suit the purposes of a psychologically 'local' problem and the convenience of some traditional practice, the most universal measurement, and that which alone has promise for the new task of bridge building between different disciplines, is the interactive measurement.

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THE AWARENESS OF SELF AND THE STRUCTURE OF THE EGO

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I. Introduction

Professor Allport's presidential address to the Eastern Psychological Association on *The Ego in Contemporary Psychology* (1), with its brief, but exceptionally able review of the main conceptions of the ego and its summary of cogent experimental studies, will undoubtedly serve as a landmark in the history of psychology and as a rallying and focussing point for further studies in ego psychology.

Allport considers eight main conceptions of the ego: "(1) as knower, (2) as object of knowledge, (3) as primordial selfishness, (4) as dominator, (5) as a passive organizer and rationalizer, (6) as a fighter for ends, (7) as one segregated behavioral system among others, (8) as a subjective patterning of cultural values" (1, p. 459). He writes that "We cannot (yet) say whether these eight conceptions reflect irreconcilable theories, whether they shade imperceptibly into one another, or whether they are all ultimately to be subordinated under one inclusive theory of the ego (1, p. 459). Professor Allport adds his belief that recent experimental studies favor the last possibility and comments that "it is my belief that in most of the experiments I shall report one finds that the ego is acting in several, if not all, of the eight capacities I have listed" (1, p. 459).

Professor Allport's paper is, in many ways, challenging as well as provocative. For, while everything seems to point to one unitary ego rather than to a number of different referents, each sometimes called 'ego,' Allport does not indicate what the "one inclusive theory of the ego" might be. In the section of his paper dealing with 'The Nature of the Ego,' he points out that the various conceptions under consideration and the views of various writers on the subject have much in common. Specifically, he lists the following points of agreement: (1) The ego is less embracing than personality. (2) It is in proximate relation to the external world. (3) The subjective sense of the ego (self) varies greatly from time to time. He adds a fourth point which he regards as of great importance, that the ego is customarily preoccupied with the future. But he does not tell us, even hypothetically, what the ego *is*.

Allport's paper is thus a challenge to psychological theorists to provide "one inclusive theory of the ego," and the present writer, for one, believes that this challenge can, at present, be met.

What, however, can be expected of such a theory? Certainly not that each of the main conceptions should find an equally important place in the theory, nor even that these conceptions be accepted without modification. These conceptions have, after all, been advanced by individuals of varied interests and competences. What can be expected is that the theory make clear what each of these conceptions is after, and that the significance of each of the conceptions should emerge from the theory and have some 'organic' relation to it. It may further be expected that the theory perform a similar task with regard to the four points developed by Allport in his consideration of the nature of the ego and that, perhaps above all, it make clear the meaning of 'ego involvement.'

II. AWARENESS OF SELF

We take, as a starting point in the search for a theory of the Ego, two rather obvious, but nonetheless important, facts:

First, the fact that everyone, with the possible exceptions of infants, some philosophers, and some psychopaths, is aware of one's self. The possibility of exceptions is freely admitted, because exceptions would not be critical for the argument. Actually, however, it is doubtful whether many of the seeming exception are genuine. There are many disorders of self-awareness, but such disorders do not constitute an absence of awareness of self. Similarly, with regard to the philosophers, it is quite evident that Hume, for example, despite his denial of its reality, did have an awareness of self. His argument did not simply lead to his discovery that he, of all people, had no self. Rather, he importuned his readers to examine their own awarenesses and see whether they did not observe what he himself observed when he examined his awarenesses. A color-blind person does not, on discovering his deficiency, confidently invite others to investigate themselves and see that they, too, are lacking in awareness of color; his discovery is likely to come with a shocked realization that he is missing something which other people are experiencing. In other words, Hume knew perfectly well what other people were talking about when they spoke of their selves and he also knew that he was talking about a corresponding referent in his own awareness. Hume did not try to prove that there was no awareness of self, but rather that this awareness was illusory or delusory, that there was no self to be aware of. We shall return to Hume's difficulty with regard to the reality of the self, but at the moment, we are only concerned with the point that awareness of self is a common experience of mankind.

Second, the fact that the self is not identical with the ego. If it were, then the knowledge of the ego would be as immediate as the knowledge of the self. It is true, of course, that the word 'ego' has been used as a synonym of 'self.' Psychologists, however, generally have something else in mind when they speak of the ego. For, while each individual recognizes himself, he has no immediate knowledge of ego processes such as repression or self-actualization. To be sure, he may know that he, John Doe, is selfish, or domineering, or submissive. But, if you said to him that it was not he, John Doe, who was selfish and so on, but his self that was selfish or domineering, he would regard you with, to say the least, suspicion. It is not selfishness or submissiveness of which the individual is aware when he is aware of his self; it is something quite different.

What that *something* is, everybody knows from a *knowledge of acquaintance*. The trouble began when philosophers, such as Hume, and numerous psychologists tried to transform that *knowledge of acquaintance* into *knowledge about*, particularly when they attempted to localize that something.

Now, in order to localize something, it is necessary to do so with reference to something else. Hume, and James, and Titchener, and many others fell into the pitfall of attempting to localize the self with reference to the body. Why this is a pitfall, will be explained shortly. Suffice it to say for the moment that the foregoing found no self, but only various bodily sensations which, to add to our troubles, were not always the same. In this extremity, Hume flatly declared that there was no *one* thing, nor even a group of intrinsically related things, that could be called the self. This, despite the almost uni-

versal experience of mankind to the contrary. Nor was there any ready explanation forthcoming as to why people (including Hume who was talking, as we have seen above, about his self) related these intrinsically unrelated sensations.

It was Köhler who, according to Koffka (11, p. 322), was inspired to localize the self, not with reference to the body, but with reference to the environment. This, as a matter of fact, is the way people will spontaneously localize themselves [1] and Köhler's formula is so obvious that it sounds silly even to say it. Yet, because of the seemingly great philosophical problems involved, it is necessary to elaborate upon the obvious.

Koffka presents Köhler's argument as follows:

> It starts with the behavioural environment: . . . space is not only in front and at the sides, space is also, though less articulated, less clearly defined, behind. . . . Now then, Köhler's argument continues, what is there between the last thing just in front and the behind? Is space absolutely empty there? The answer is: Certainly not; here, between the 'in front' and the 'behind,' is that part of the behavioural world which I call my Ego. It has a very definite place in that world, and well-defined, if variable, boundaries. From this argument we can take the following answer to our question: 'in front,' 'to the left and right,' 'behind,' and 'above and below' are characteristics of space which it possesses with regard to an object which serves as the origin of the system of spatial co-ordinates. This object, then, is functionally different from all others, inasmuch as it determines fundamental space aspects (11, p. 322).

Koffka's presentation is unnecessarily complicated and, particularly as it is couched in terms of a conceptual framework which is still foreign to a large body of American psychologists, it seems worth while to translate it into more familiar terms.

Elsewhere (5), the present writer, following a respectable psychological tradition (particularly among American psychologists), has briefly presented the point of view that *awareness* refers to the discriminatory activity of the organism. Applying this view to the present problem, we may say that the organism (in this case, we can only speak definitely about *man*) is aware of (*i.e.*, it discriminates) space-in-front, space-behind, space-to-the-right-and-left, space-above, and space-below. In between these spaces, it is aware of (it discriminates) something else. That awareness of something in between *is the awareness of self*.

This seems so obvious and clear that it is astonishing that anyone should have gotten into trouble on this score. Yet, there are three reasons for the trouble which can more readily be comprehended in our terms than in Koffka's.

First, there is the problem of two things being in the same place at the same time. In between the 'in front' and the 'behind,' is the body. Yet, experientially, the awareness of self is not the awareness of the body. In terms of the logical principle that two things cannot occupy the same space at the same time, this seems impossible. *Where* in-between is the self? It is not the body and it is not *in* the body, for the internal bodily space is well accounted for in terms of various organs and so on. Where, then, is it?

The impossible paradox is readily resolved if we remember that the awareness of self and the body are not the same order phenomena. The turning of a wheel has a spatial locale, but it has no spatial dimensions. The turning of a wheel occurs in space, but it is not

[1] Ask someone where he is and he will answer in terms of environmental coordinates. He will never say, "*I* am here, in the form of certain sensations around 'my' eyes and throat." To press him further, creates an artificial situation.

the wheel, nor is it in the wheel; it is a relation between the wheel and various objects. Similarly, awareness, any awareness, is a relation between the organism and various objects which it is discriminating.[2] The awareness of self is no exception. Being, like all awareness, an action, it has a *locale,* but no physical-spatial dimensions.

Second, there is the common failure to distinguish between the object of awareness and the content. The *content* refers to *what the organism discriminates* and the *object* refers to *what is there to be discriminated.* The content is the discrimination, the object is, to use Tolman's terminology, the *discriminanda* (the properties of objects or situations whereby they can be discriminated). It is important to distinguish the two because they do not always correspond perfectly. Thus, with regard to eye movements, the object of discrimination is the jerky eye movements, the content of discrimination is the smoothly moving eyes. Or, with regard to the Müller-Lyer illusion, the object of awareness is the pair of equal lines, the content is a pair of unequal lines.

This is important with regard to the awareness of self. For the object of awareness is indisputably the body. That is what is in-between. Yet, the content of awareness is not the body; nor does it correspond perfectly to the body. Such a lack of correspondence between object and content is not unusual and it is one of the tasks of psychology to explain such discrepancies wherever they do arise. In this particular instance, it is not especially hard to explain the discrepancies, but that will not be attempted here.[3]

Earlier it was stated that the *awareness* of self and the body were phenomena of different orders. It may now be added that the *self* and the body are also phenomena of different orders: the self being a content of awareness and the body being a *physical* reality.

Third, there is the frequent failure to realize that different contents may be functionally equivalent, just as different things may be functionally equivalent. Earlier, we mentioned that there was no ready explanation forthcoming as to why the different experiences which are arrived at in the attempt to localize the self within the body are normally experienced as *my self* and not simply at one time, as a tension in the eyes and, at another time, as aching feet. No explanation *can* come forth so long as we regard these experiences as disparate phenomena, without any coordinating principle. An explanation is, however, readily found when we realize that the awareness of self is an awareness of something between the 'in front' and the 'behind.' What these 'different' experiences have in common is that they all carry the burden of being that something in-between. In this respect all of these experiences are the same, even though in other respects they are different. This is what accounts for the apparent fluctuations in the 'spatial' boundaries of the self. My hand can very readily be experienced as being outside of myself if it is discriminated as belonging to the 'in front.'

These, then (the failure to realize

[2] It will be recalled that Koffka does attribute spatial dimensions to the self, but this is space in the behavioral environment, which is a different order space than that of the geographic environment (ordinary physical space). Similarly, when Boring in his *The Physical Dimensions of Consciousness* (3, p. 28 and Chap. 4), speaks of *extensity* as one of the attributes of awareness, he cannot mean that the extensity of awareness has reality in *physical* space. This point will become clearer when we contrast the content of awareness with the object of awareness (see below).

[3] The approach to the problem of discrepancies is in terms of the answer to the question: Under what conditions must a given discrimination be made?

that body and self are phenomena of different orders, the failure to distinguish between the content of awareness and its object, and the failure to realize that different mental contents may be functionally equivalent), are the main reasons for the intellectual muddle that has arisen with regard to the self. There seems, however, also to be some difficulty with Köhler's conception.

The difficulty arises from the possibility of identifying oneself with some other person or of identifying some other person with oneself.[4] In both cases, the awareness of self seems to cross a spatial gap separating the person from the object of identification. It seems as if "here I am, there I am not, and there I am again." What happens, in these cases, to the discrimination of something between the 'before and behind'?

This difficulty disappears with a closer inspection of these phenomena. The writer frequently is unable to identify himself with the characters of a motion picture because of the intrusion into his awareness of what is actually going on to the right, left, and behind. The extent of this awareness is not appropriate to the 'in-front.' The more successful the identification, the less of a 'clash' there is between the 'in-front' and the other directions. In other words, in successfully identifying oneself with the characters in a motion-picture, one does not experience oneself as being in one's seat, but as being there, in the picture-world; and, to do this successfully, all directions must be appropriately experienced. Similarly, if one identifies some love object with oneself and some danger threatens that object, then one experiences the danger as occurring 'here,' where one oneself is. With lesser degrees of such identification, one may experience the danger as being both 'here' and 'there'; but the more the danger is experienced as being 'there,' the less of such identification there is.

Köhler's solution of the problem of the awareness of self and of the phenomenal nature of the self holds. It should be clearer than ever, however, that the awareness of self is not an awareness of the ego, that the ego is not simply 'something' between the 'before and behind.' What, then, of the ego?

III. THE STRUCTURE OF THE EGO

Elsewhere (4) [5] the writer has developed a set of concepts which are very relevant here: the perpetuation, derivation, and imbrication of motives.

In terms of the concept of the perpetuation of motives, any frequently recurrent motive (organically determined or derived) which meets with some frustration results in the development of expectations of its recurrence and the development of a need on the part of the organism to prepare for its recurrence. In other words, the motive becomes broader in its implications and, in this broader sense, continues to function even though the *critical* conditions for its existence are no longer operative. Such a *perpetuated* motive can never,

[4] These are both phenomenally and dynamically different. The first may be exemplified by an identification of oneself with some character in a motion picture, the second by the phenomenon of 'falling in love.'

Both forms of identification are dependent on ego processes. We are, however, only concerned here with the relation of these phenomena to the awareness of self.

[5] The writer makes no claim to originality in connection with these concepts. With the possible exception of the concept of perpetuated motives, they have been clearly anticipated by other psychologists in various terminologies and contexts. He is not aware, however, of their having been explicity put together in this way for the purpose of elucidating problems of personality and ego structure. For the concept of derived motives see, *e.g.*, Tolman (12, 13). For the imbrication of motives, see Horney (9). With regard to perpetuated motives, see Hunt (10).

in a very genuine sense, be satisfied; for it is always pointing toward the future and is not concerned with present circumstances; its goals are not attainable. It is only in terms of such motivational perpetuations that we get something that endures and is not transient like ordinary behaviors and awareness.

In terms of the concept of motivational derivation, any environmental situation which requires that the organism have commerce with a given means in order to attain a given end, or that the organism satisfy certain conditions in order to attain that end, results in the development of a need on the part of the organism to have commerce with that means or to satisfy the necessary conditions, provided that it already has a need to attain that end. Such motives, like organically determined motives, may also become perpetuated and enduring, provided that the conditions for such perpetuation are satisfied.

More complex motivational derivations are also possible, as where a derived motive is an indirect result of the derivational process previously described. Here belongs the problem of the psychoanalytic superego, but that is too complex a problem to discuss at this point. A simpler case is to be found in the indirect derivation of elimination motives. So long as excretion takes place spontaneously and without interference, as the 'need' arises, there can be no elimination motives. With the development, however, of a need for control, there occurs an interference with spontaneous elimination and a consequent building up of bodily tensions leading, in this case, to a genuine appetitive drive.

By the imbrication of motives is meant the fact that perpetuated motives do not remain isolated from one another, but rather that they influence each other in the most complex ways. The same perpetuated-derived motive may derive support from a number of parent motives. Thus the need for approval may be related to a host of other motives. Moreover, the derivation of motives involves not merely transitive or simple hierarchical relationships, but circular relationships as well. Thus, while the need to earn money may be in part derived from the need of the approbation of others, the need for approval is, in turn, in part derived from the need to earn money.

The result of the process of motivational imbrication is the development of a structured motivational system, a genuine Gestalt, in which anything which affects any part of the system affects the system as a whole. The fact that the motives involved are perpetuated motives implies that the system is relatively enduring, although not necessarily unchangeable. In the paper to which reference has already been made, the writer suggested that such a system satisfies the essential logical requisites of what may properly be called personality. Here, we shall consider the relation of the above described development to the ego.

The essential key is provided in a reconsideration of the foregoing remarks concerning the structurization of motives. It has to be observed that motives are polarized processes. Consider, for example, the dependence (need to be cared for) motive. Such a motive is bipolar.[6] At one pole are the persons or institutions from whom something is demanded or expected. With reference to these persons or institutions, appropriate actions have to take place such as will elicit the demanded or expected solicitude and care. At the other pole, however, there is something else. It is

[6] *Cf.* (2). Bateson is, of course, not talking of motives, but of *social motifs, i.e.*, of the environmental setting in which various motives develop.

not the body, except insofar as the 'body image' is incorporated into the awareness of self. At the other pole of the dependence motive there is, precisely, the self of which one is aware. The awareness of self is, in fact, part and parcel of the dependence motive.

The polar situation is even clearer with regard to the so-called self-preservative motive. Here the motive is, basically, unipolar, although motives derived from self-preservative motives may also have outer poles. Again the essential pole is not the body, although here a great deal of confusion reigns, particularly among laymen, but too often among psychologists. Even Freud, who so clearly recognized the derivation of the self-preservative motive (*via* the cathexis of the self), could sometimes, for example, confuse hunger as a self-preservative motive. With the development of the theory of the life and death instincts, Freud made a similar error in assuming that reproduction was intrinsically a psychological affirmation of life. The fundamental error is the confusion of the *consequences* of an act with its *aim*.[7] The writer remembers a philosophy instructor who argued that "dogs have a philosophy of life, for why should they copulate if they did not value offspring?" This instructor could not, rationally, conceive of copulation apart from reproduction. The error would have been just as great if he had said, "Why should they eat, if they do not value life?" The homeostasis of physiological processes is not a motive although it may sometimes give rise to appetitive drives. It is true, of course, that people sometimes eat in order to preserve the bodily health, but such a motive is itself *derived from* a self-preservative motive; it is not, in itself, a motive of self-preservation; and, often enough, eating has no relation to either self- or body-preservation.[8]

Bearing in mind, then, the many confusions which arise in connection with self-preservation, we may again say that the essential pole of the self-preservative motive is the self of which the individual is aware, not the body. It thus often happens that, to the individual, self-preservation may be perfectly compatible with the destruction of the body.

As with the dependency motive and the self-preservative motive, so with the prestige motive and numerous others; these motives are built up around the awareness of self and do not have any meaning without reference to the self.

We have, then, if the above argument is correct, a structured set of interrelated motives centering about the awareness of self. It is now suggested that *this structure is the ego.*

It must be remembered that not all perpetuated motives, certainly not all motives, necessarily have direct relevance to this structured set of motives. Or, to express the matter differently, the frustration or alteration of any one motive need not be as critical for the total structure as would be the frustration or alteration of another motive. The less direct relevance a given need has for the total structure of self-centered needs or the less critical a role it

[7] In defense of Freud, it may be urged that he often had reason to believe that the seemingly *accidental* consequences of acts were actually the *intended* consequences. Each case, however, as Freud well knew, has to be judged on its own merits. The frequent coincidence of seemingly accidental consequences with intended consequences justifies, as a working hypothesis for the further exploration, the assumption that *any* consequence may have been *intended*. A careful distinction must, however, be drawn between a working hypothesis for further exploration and a conclusion that the hypothesis is correct.

[8] Freud realized the difference between hunger and eating for pleasure.

plays with reference to the total structure, the more peripheral it is to the ego.

It may still be a problem for investigation whether, and to what extent, the ego-involvement of needs is correlated with their intensity, particularly in the case of peripheral or non-ego-involved needs. The need to solve a problem or to earn a high score on a test may not be of much moment, for example, from the point of view of critical ego needs, but can it, under these circumstances constitute a strong motive for the organism? Can appetitive hunger or thirst become so intense that the ego structure becomes temporarily irrelevant in the psychological economy of the organism? If this does happen, in the case of appetitive drives, can it also happen with various derived motives?

It must also be remembered that the ego structure may be more or less stable. If it is composed of mutually complementary and compatible needs, it is stable and resistant to any change which would destroy the motivational equilibrium. If it is composed of mutually incompatible and dystrophic needs, it is easily disrupted. Any disruption of the ego, however, must follow the lines of cleavage that are natural to the particular structure. In any case, it cannot be stressed too much that a unitary structure does not necessarily imply unity within that structure. The different motives involved may be mutually supporting but they may also be mutually antagonistic. One of Horney's great contributions is her analysis of areas of conflict within the ego structure (8, Chap. 10).

Finally, it should be noted that, by virtue of the central role it plays in the ego structure, the significance of the self becomes enormously enhanced. From the center of the person's awareness, it becomes the center of his existence. From an item, in a universe of observable items, it becomes a supreme value and the core of an evaluative system.

IV. Main Conceptions of the Ego

We may now return to Allport's list of eight main conceptions of the ego and to his four points concerning the nature of the ego, to see how they fare in terms of the theory.

1. *The ego as knower.*—It is the organism that knows, the organism that discriminates, the organism that is aware. As Allport himself writes (1, p. 453), "it is enough, says James in effect, to admit that knowing goes on. A separate knowing-ego is not a necessary assumption."

At the same time, we may note that there are numerous cognitions and cognitive functions bound up in the ego structure. The perpetuation of motives is dependent upon the development of expectations and anticipations of the recurrence of episodic motives. The derivation of motives is dependent on the apprehension of means-end relations. The functioning of all motives is dependent on the awareness of the environment, save, perhaps, for the effect of random restlessness. The ego system, therefore, is not merely a motivational system, but, by that very fact, a cognitive system as well. If the theory is correct, then this much is valid concerning the ego as knower.

2. *The ego as object of knowledge.*—As Allport deals with this conception, it is concerned with the self, not with the ego. We have already dealt with this problem at greater length above. The ego is not, spontaneously, known; it becomes the object of knowledge in the same way that any phenomenon becomes known in the course of scientific investigation.

3. *The ego as primitive selfishness.*—We have already noted that the self, by virtue of its central position in the ego structure becomes a supreme value and

the core of an evaluative system. By implication, the ego is selfish. Yet, a great deal would be missed if we let the matter go at that. There is possible, if we may so express ourselves, a selfish selfishness and an altruistic selfishness, a short-sighted selfishness dominated by the pleasure principle and a long-sighted selfishness dominated by the reality principle. In addition, because the self of which the individual is aware is not identical with his body, various transformations of the self are possible, through identification, *etc.*, which destroy the popular meaning of selfishness. In general, we may add, the more fully developed the ego, the less selfish (in the popular sense) the ego is. This is so because there are so many motives involved which must not interfere with one another. Through the interaction of motives, greater scope, and consequently, less essential selfishness is demanded.

4. *The ego as dominance drive.*—Without denying the reality of dominance drives, we may say that this conception mistakes a part for the whole, an ego motive for the ego. How important a role dominance drives play in the ego structure will depend on many factors: the specific pattern of ego development, the importance of dominance in the psychological economy of the individual from the point of view of the requirements for coping with the environmental situations that the individual has met, and the other perpetuated motives developed by the individual. That is to say, the dominance drives are not necessarily basic in the ego structure.

"The negative states," Allport writes (1, p. 455), "of anxiety, insecurity, defensiveness, resistance are just as truly indicators that whenever the ego is debased there arise impulses for its defense and restoration to status." With this statement, our theory demands whole-hearted agreement. For, such negative states carry with them threats of frustration and deprivation with regard to the whole motivational structure that comprises the ego.

5. *Ego as a passive organization of mental processes.*—This conception is erroneously ascribed, by Allport, to Freud. Allport misinterprets Freud's discussions of the relative 'smallness' of the ego when faced with a world not built according to its specifications. He writes (1, pp. 455–6): "The ego, having no dynamic power, tries as well as it can to conciliate and to steer the warring forces [id, superego, and environment], but when it fails, as it often does, it breaks out in anxiety." As a conciliator, it is not passive and, according to Freud, it is the ego that represses and rationalizes and so on. Freud is talking about the ego when he writes, "A reaction which combines features of both these is the one we call normal or 'healthy'; it denies reality as little as neurosis, but then, like a psychosis, is concerned with effecting a change in it. This expedient normal attitude leads naturally to some active achievement in the outer world and is not content, like a psychosis, with establishing the alteration within itself; it is no longer *auto-plastic* but *alloplastic*" (6, p. 279). This is not a passive victim of external forces. Similarly, when Freud writes, "One might compare the relation of the ego to the id with that between a rider and his horse" (7, p. 108), he is talking about the *mastery* of the id by the ego. If he adds that "all too often . . . the rider is obliged to guide his horse in the direction in which it itself wants to go," *all too often* does not mean *always*, and the obligation does not imply a cessation of struggle for mastery. The therapeutic objective (7, pp. 111–112) "to strengthen the ego, to make it more independent of the super-ego, to widen

its field of vision, and so to extend its organization that it can take over new portions of the id," likewise, does not deal with the ego as a passive organization of mental processes.

Allport quite properly rejects the passivity theory, to whomever it may be attributed, in favor of an activity theory. In terms of the theory we are expounding, all of the above mentioned processes can be comprehended in terms of the interplay of ego motives and factors external to the ego structure. To discuss them in detail would take up too much space.

6. *Ego as a 'fighter for ends.'*—This is implicit in the theory we have advanced of the ego as a dynamic system of motives.

7. *The ego as a behavioral system.*—Allport is here concerned with behavior that is ego-involved. Concerning this we have already commented above. We may here add, however, that in a very genuine sense the ego, as we have developed the theory, is a behavioral system. For motives are incomplete behaviors. It is in the nature of *perpetuated* motives that they are never completed.

8. *Ego as the subjective organization of culture.*—It is intrinsic to the concept of perpetuated derived motives that these motives and the cognitions that are implicit in these motives refer to the environment as the individual finds it. How he finds it is, to a large extent, determined by the culture in which he resides. It is not true, however, as Allport states (1, p. 458) that "By stressing the cultural content of the ego, these authors in effect eradicate the artificial Freudian distinction between ego and superego." The distinction between ego and superego is not in the fact that one has cultural content and the other has not, but in the mode of derivation and of functioning of the motives involved and in the relations to the environment of the two systems.

Turning to Allport's four points concerning the nature of the ego: The ego *is* less embracing than personality, for the latter includes motives with respect to which the self is not a critical pole. As most psychologists use the word 'personality,' it also includes a good deal more. The ego *is* in proximate relation to the external world, for it is not merely a motivational structure, but, by that very fact, a cognitive structure as well. The subjective sense of self *does vary* from time to time, but in terms of specific content, not in terms of functional significance. Finally, the ego is customarily preoccupied with the future, because that is the nature of motives, in general, and of perpetuated motives, in particular.

In conclusion, we would like to rephrase the argument. The experiments which Allport cites point very strongly to the existence of *something* which may be called 'the ego.' These experiments, to a slight degree, and psychoanalytic and related clinical investigations, to a very large degree, have thrown a good deal of light on the workings of this something. Yet, *what* is this mysterious something which works in these diverse ways? Until this question is answered, all the rest seems vague and empty. In this extremity the theorist is called upon to answer the question, "Well, what *can* be there?" To this question, it is clear, the answer is not, 'the self.' For the self is a content of awareness; it has no reality apart from awareness and it does not correspond closely to the real *object* of awareness. Such a *content* cannot be selfish, or strive for dominance, or look to the future. If there is something which does these things, it must have reality apart from awareness. What we have done here, is to suggest *what* that something *can* be and, until

someone suggests something else that it *can* be that fits the facts better, we must assume that our description of the ego is correct: the ego is a motivational-cognitive structure built up around the self.

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THE NEUROPHYSIOLOGY OF POST-CONTRACTION

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I. Introduction

The manner in which the arm moves involutarily, following a sustained tension exerted by the arm, has long been a matter of scientific observation. The introspective fact that the arm "seems to float" was first described by Salmon, and is widely known as the *Kohnstammsche Phänomen* (5, 21). Although other systems of muscles are also found to behave similarly (21, 33, 34), this is the response that commonly is taken to typify involuntary muscular behavior.

Other names have become associated with this response or the method of its elicitation. The mode of its demonstration was early designated the *Katatonusversuch* and the *Deltoideusversuch* (30, 47). The concept of after-discharge has been widely applied in this connection (9, 23, 28, 54, 55). The response also is known as: the automatic contraction (42), the residual contraction (44), the automatic movement (48, 49, 50), the after-movement (13, 33, 34), the involuntary contraction (21) and post-contraction (4, 5, 29). Present considerations justify our adherence to the name post-contraction.

The earliest studies concerning post-contraction, which called attention to this interesting motor after-effect, also indicated the diversity of conditions under which it may be elicited. They also suggested the clinical significance of the phenomenon.

Post-contraction now encompasses an extensive literature. Recent experimental studies have been concerned with various psychophysical relations. The manner in which the angular displacement of the arm (the magnitude of the involuntary response) varies as the intensity, or the duration, of the preceding (voluntary) exertion is increased has been established (5, 10, 11, 12, 29, 33). The relation between amount of displacement and intensity of stimulation for the range of 20–6400 gr. is shown to be a power function (29). Intensity functions have been found to exhibit cyclic behavior when measurements are made at intervals of time after those for an initial function are taken; *e.g.*, successive presentations of the same weight at intervals (1, 3, 5 and 10 minutes) yielded an oscillatory post-contractional effect, *i.e.*, regularly recurring diminished and augmented responses to the same stimulus, although the regularity of the successive troughs and crests was somewhat obscured due to interaction of successive effects of stimulation (4). The nature of the relation between variability of response and amount of angular displacement of the arm has been graphically depicted; *viz.*, in relation to angular displacement, organic variability increases at first, passes through a maximum, decreases, passes through a minimum and then increases again, indicating the existence of two distinct proprioceptor distributions (29). The fact that post-contraction is elicited contralaterally has been established; *e.g.*, responses for the right arm, obtained 3 min. after a weight of 2 kg. had been supported by the left arm for 60 secs., resulted in a depressed function for all intensities used between 550 and 4550 gr. (5). Although numerous psychophysical relations have been definitely established, the physiological basis of the phenomenon has not yet been so clearly brought to light.

The muscles involved in the phe-

nomenon have attracted interest from the very beginning. Although first designated the 'Deltoideusversuch' as if it involved only the *deltoid* muscle, Allen and O'Donoghue state that movement of the arm through a particular arc is not restricted to contraction in the deltoid alone, but involves a number of muscles: *supraspinatus, triceps, anconaeus, extensor digitorum longus, extensor digitorum brevis* and *extensor pollicis* (5, p. 217). Once it was suggested that the response might be restricted to extensor muscles (*cf.* 47), but now investigators recognize that it may be elicited in skeletal muscles generally, requiring only "suitable mechanical aids to render its effect visible" (21). Matthaei has presented a plate illustrating the manner in which the after movement may be elicited in at least twenty different muscle systems scattered widely in various parts of the body (33). Allen and O'Donoghue also subscribe to the view that it may be secured in widely different muscles (5).

Almost every aspect of the problem is greatly in need of development. One aspect is the specification of the mechanism by which a system of muscles is brought into a sustained involuntary after-contraction which persists for many seconds and even for one or two minutes (45). The method of electrical recording has been utilized in an attempt to isolate the conditions assumed to be responsible for the eventuation of the effect. Some writers have contended that this involuntary response represents an instance of action-currentless contraction in skeletal muscle (42, 43, 51). But action currents are associated with the muscles involved in post-contraction, as a more adequate control of instrumental conditions has revealed. Forbes, Baird and Hopkins, for instance, have recorded action currents during post-contraction in human subjects in the biceps, the triceps, the wrist flexors, the abductors and flexors of the hip, the flexors and extensors of the knee and the muscles of the neck (21, p. 97).

The object of the present paper is to consider the general nature of the mechanism underlying post-contraction. The consideration will comprise an evaluation of various theories found in the literature in terms of recently acquired neurophysiological information.

THEORIES OF POST-CONTRACTION

The theories to be evaluated are (1) suggestion, (2) contrast, (3) catatonus, (4) fatigue, (5) idiomuscular action, (6) muscular tonus, (7) antidromic action, (8) reflex action, and (9) delaying action in the central nervous system (neurone detours, delay paths, self-exciting reverberations). The arguments submitted in support of each will be presented, and their merits indicated.

Suggestion.—Many writers have committed themselves negatively in regard to this view, pointing out that suggestion is not the primary cause. The inadequacy of suggestion to account for the phenomenon is indicated in the reports of certain subjects concerning the initial demonstration of the phenomenon. The complete surprise which is manifested when the subject's arm begins to rise involuntarily is strong evidence against the case of suggestion (21).

Contrast.—Fechner noted long ago that when he shifted from lifting heavy weights to much lighter weights, a very positive experience of lightness which he designated 'negative weight' was experienced (14). This observation became the basis for experiments on 'motor adaptation' by Müller and Schumann (36) and by Steffens (56). Myers was later to use these observations as the prototype for experiments in 'motor attunement' (37).

Allen and O'Donoghue also acknowl-

edge the relation existing between negative weight and post-contraction (5). Although Fechner's experience of 'negative' weight is not to be identified with the *Kohnstammsche Phänomen,* Matthaei intended to show that it is the subjective counterpart of the 'after-movement' (34). In attempting to account for 'negative weight' in terms of events in the periphery, Fechner appealed to the principle of contrast. The lighter weight seemed negatively weighted (lighter than usual) as if in contrast to the persisting effect of the previous activity involving heavier weights. Matthaei, however, aimed to show that the 'after-movement,' and its subjective counterpart, are determined centrally (33, 34). He conducted experiments in which the subject was asked to lift equally with the two hands on a double tension scale, consisting of two like tension springs, immediately following an exerted tension (*e.g.*, lifting a weight) by the muscles of one of the arms. He advanced two principal arguments against the view that the 'after-movement' can be explained essentially in terms of sensations of contrast. The first was that the absolute magnitude of the error does not depend on the 'normal' pull. The second principle argued that the after-movement continues to be manifested when sensation is eliminated by the injection of novocain (20 cc. of a 1 per cent solution, with adrenalin) in any one of four places in the deltoid muscle.

The fact that post-contraction is said to be manifested contralaterally is consistent with a central view as well as a peripheral view (5).

Catatonus.—Although Kohnstamm reported that the effect is diminished or lacking in cases of tabes dorsalis, and thus seemed to indicate that after-movement is influenced by central factors, he emphasized that the primary origin of the phenomenon is in the muscles (30). He attributed it to a mechanical action of the muscles which immediately followed and outlasted the antecedent voluntary contraction. He likened it to tetanization of the muscles, and designated it 'Katatonus.' That he called his study the *Katatonusversuch* possibly indicates the importance he attached to the muscular component.

Csiky points out that the conception of a mere mechanical action of the muscles is inconsistent with the fact that the antecedent voluntary contraction, which is the stimulus for the effect, is followed by a latent period of 2 or 3 secs. (13). The view that the response is essentially like tetanization of the muscles also does not harmonize well with other observations, *e.g.*, the fact that the response is readily elicited by a relatively small stimulus, and that there is no evidence of a transition from partial to complete 'Katatonus' when a wide range of stimulus-intensities is used.

Fatigue.—It has been suggested that the effect is a conspicuous property of fatigued muscle (13), although little tendency has been manifested to regard the 'fatigue' theory as the primary cause.

The fact that the response to the greater of two degrees of submitted tension is the more pronounced seems truly enough to harmonize with the notion of fatigue. However, to explain post-contraction essentially in terms of fatigue entails the assumption of an extraordinarily rapid rate of development of fatigue products. Furthermore, the fact that the phenomenon is readily elicited when the organism is well rested seems to impose upon this theory the additional assumption of constant availability of residual fatigue products in a state of ready and immediate availability for transmutation to a motor effect and in an amount sufficient for sudden use.

Now common experience hardly warrants the inference that fatigue manifests itself in this specific way. At least, Matthaei has asserted that fatigue in the muscles constitutes a disturbing factor and is not to be assigned as the cause (33, p. 104). His study shows that the reflexes of the frog evince diminished after-effects when stimulation exceeds a certain limit. The independence of fatigue and persisting effects of excitation is indicated by the fact that the latter was observed to vanish more quickly than that of fatigue when the latter was present in very small degree.

Idiomuscular action.—Csiky assumes that the *Kohnstammsche Phänomen* is an 'idiomuscular' effect (in fatigued muscle),—a purely muscular response (13). He secured the response by faradic stimulation of the muscle at its motor point as well as by means of the more usual mode of voluntary contraction. Pereira also supports the view that post-contraction has a muscular origin (42, 43). He correlates action potentials secured from the deltoid muscle with *movement per se,* not with contractile changes in the tissues. Action potentials attended both passive and active elevation of the arm, but were not secured during sustained contraction at the maximum, nor when an obstruction was used to arrest the response. On the basis of such findings, Salomonson attempted to distinguish between muscular action which is action-currentless (post-contraction) and muscular activity which is attended by electrical effects that may be recorded by a galvanometer (51).

As stated above, however, subsequent work has not borne out the contention that the muscular activity upon which the post-contraction depends is action-currentless, and the 'idiomuscular' view of post-contraction has lost caste. Forbes, Baird and Hopkins point out that Pereira failed properly to correlate movement of the arm *per se* and the attending electrical records (21). They indicate also that galvanometric recording under Pereira's conditions is very difficult and uncertain. Not only is the skin a poor conductor through which to secure such records from muscles when electrodes are attached to the skin overlaying the muscles, but the quartz string in the instrument used by Pereira has great inertia owing to a very high tension. Very telling evidence against Pereira's contention is the fact that with the same instrument Forbes, Baird and Hopkins succeeded in recording action currents through the skin over the muscles involved in the 'involuntary post-contraction' and showed that electrical effects appear *irrespective of motion.* Other investigators have also succeeded in recording electrical changes through the skin from muscles involved in post-contractional activity (44, 52, 53). That Pinkhof found the same rhythm in the electrical excursions for post-contraction and for voluntary contraction casts doubt on the distinction between muscular action which manifests action-currents and muscular action which is sometimes said not to do so (44).

Although Csiky secured the response by faradic stimulation at the motor point of the muscle, and used this as the basis for the argument that the response is primarily 'idiomuscular,' both Kohnstamm and Matthaei failed to secure post-contractional response through (direct) faradic stimulation of the muscles. To explain why Csiky did secure the response by electrical stimulation, Matthaei suggests that the voluntary contraction of the muscles which is a necessary antecedent of the post-contraction must have been brought into play along with the faradic stimulation (33, p. 104).

Muscular Tonus.—Rothmann argued that the phenomenon is due to excita-

tory processes in subcortical nerve centers which are concerned with the maintenance of a state of increased tonus after the voluntary exertion is terminated (47). He submitted the clinical observation that the manifestation is reduced, or lacking, in patients who have cerebellar lesions. This prompted him to suggest that the controlling nervous center was not in the motor cortex, as Salmon first suggested, but in the cerebellar cortex. Although it is probably true that a change in muscular tonus attends the antecedent voluntary contraction which is employed to elicit the post-contraction, muscular tonus does not usually express itself in the highly specific way that must be assumed if change in tonus were to constitute the primary cause of the phenomenon.

Antidromic Volleys.—One suggestion for the explanation of reflex after-discharge is antidromic action (23). The antidromic volleys pass into the spinal cord and affect the state of the motoneurone. "If the intact efferent nerve is stimulated immediately after impulses have begun to reach it over an associated afferent nerve, the antidromic volleys will block the reflex discharge for a period ranging from 20 to 60 sigma. These antidromic volleys apparently render certain motoneurones temporarily inactive, their discharge recommencing after a fresh excitatory state has been built up. . . . The first reflex discharge passing down an efferent nerve fiber tends to inactivate the excitatory state of its associated motoneurone by an antidromic action. These delayed excitatory impulses are out of phase with the after-discharges of other motoneurones; consequently, there is an almost uninterrupted sequence of contraction in individual muscle fibers following a single volley of afferent impulses" (23, p. 265 f).

A later writer, however, interprets this evidence to mean that the after-discharge depends primarily upon an enduring excitatory state in the central nervous system and that the antidromic volleys merely serve to arrest or to cancel the discharge arising from the central excitatory state which is the basis of the after-discharge (28, p. 191).

Reflex Action.—Forbes, Baird and Hopkins, and Allen and O'Donoghue have insisted that the involuntary post-contraction subscribes to the general pattern of reflex action. The phenomenon evinces a latent period of 2 to 3 secs. (metronome, 13), or 1 to 2 secs. (electrical recording, 44). The mechanism underlying post-contraction is reflex in nature, involving the transmission of afferent impulses that arise in proprioceptors to the spinal nerve centers and their reflection back over the efferent paths to the same muscular system, but it is to be emphasized that classifying the response as a reflex is not equivalent to specifying the series of events which are the cause of the gross reflex act. A modern theory of post-contraction needs to go beyond mere classification. It needs to provide an account of the response in terms of established neural events.

Delaying Action in the Central Nervous System.—The probability that post-contraction depends primarily upon a central excitation has been suggested by a number of observers, and a succession of principles has been proposed, chiefly to account for the fact that a post-contraction usually outlasts the stimulus for many seconds. But only the most general nature of the central excitation has been indicated.

Salmon suggested that a representation of the voluntary act that is used to elicit the response persists in the cerebral (motor) cortex, analogous to the persisting after effects which follow an intense sensory stimulation, and that efferent impulses are discharged from

this excitatory field (48, 49, 50). According to Forbes, the nature of the gray matter is such as to provide for elaborate and extensive paths with differing times of arrival of impulses over the final common path (19, 20), and Fulton has pointed out that after-discharge persists too long in certain spinal reflexes to be explained by the notion of neurone detours (24). Self-exciting reverberations in the central neurones might subserve these responses (20), although the basis of such reverberation has never been elucidated. Lorento de No has suggested that the key to the underlying mechanism is a difference in the length of the refractory periods of afferent and efferent neurones (31).

These suggestions regarding central excitation seem to constitute attempts to grasp the key to the solution of the problem of post-contraction, as though it were known that one and only one key existed.

THE MECHANISM OF POST-CONTRACTION

The mechanism for post-contraction is a sequence of neuromuscular events initiated when the stimulus for an involuntary post-contraction begins its action upon the particular neuromuscular system. If the sequence of neuromuscular events thus set into action were to be described in sufficient detail, the principal characteristics of a typical post-contraction would be regarded as understood.

The neuromuscular changes which take place in muscle and in nerve during various kinds and degrees of stimulation has been the subject of intensive investigation in recent years. New techniques have been devised for securing new kinds of neuromuscular information, and methods of recording have been improved to render the information obtained more useful (2, 3, 32, 35). An extensive body of information is now available and is being widely used to interpret diverse aspects of behavior. This information may be applied to the problem of post-contraction. The information regarding events in certain parts of the neuromuscular chain is not so definite as that in others, but progress toward a complete description can be made by carrying the account of the pertinent neuromuscular events as far as the available evidence permits.

The stimulus for a post-contraction, "a powerful voluntary contraction" lasting from 5 to 60 secs. (13, 21, 30, 47, 48), serves to impress tension upon a large population of spindle cells in the muscles in the arm. The degree of the tension that is induced in all of the cells which are called into activity during this act most probably is not the same, however; a strong voluntary contraction of the muscles in the arm may be presumed to impress a comparatively high tension upon a very large population of muscle cells. Action currents which have been taken from the deltoid muscle during the voluntary contraction that immediately precedes a post-contraction tend to support this conclusion (21).

Tension excites muscular cells, and is usually attended by happenings in the supplying afferent nerves. Nerve impulses (propagated disturbances) begin to be discharged over the afferent nerves very soon after a muscle begins to stretch, or to respond to faradic stimulation (22). The frequency of impulse-discharge over individual nerve fiber per unit time is an increasing function of the intensity of the stimulus that is applied to the muscle (1, 2, 3, 8). A strong voluntary contraction is prescribed for the elicitation of a post-contraction. Hence, it may be assumed that a large number of afferent nerves are conveying impulses at comparatively high frequencies during the period that the voluntary contraction is maintained.

During steady stimulation of constant intensity in a receptor-field, the frequency of impulse propagation in an individual nerve fiber does not remain constant, but exhibits a decline as a function of time (2, 6, 7, 8, 35). The rate of decline for any given intensity of sustained stimulation is most rapid initially and gradually becomes less and less. The initial rate of decline during a continued constant stimulation is more rapid for a higher than for a lower stimulus-intensity. During the voluntary act which is used to elicit a post-contraction, the impulse transmission per unit time over the supplying individual nerves must exhibit a decline that is rapid at first and gradually more and more attenuated.

The decline in the rate of impulse transmission during continued stimulation of constant intensity denotes a progressive change in the functional characteristics of the nerve during activity. Other experimental procedures also have indicated the eventuation of functional changes in the properties of nerve as a function of time during continued activity. These facts need to be borne in mind in an attempt to find the explanation of the phenomenon of post-contraction.

The refractory period of nerve has been shown to increase greatly during a continued stimulation at constant intensity (18). The capacity of the nerve to transmit impulses undergoes change owing perhaps to chemical changes in progress in nerve during activity. This is indicated in a number of known ways. The nerve utilizes oxygen at a substantially increased rate (15, 16, 17), and gives off carbon dioxide appreciably more rapidly when the nerve is brought into steady activity (38, 39, 40, 41). Heat is produced in nerve during a sustained faradic stimulation, and the amount of heat production is an increasing function of the intensity of the stimulus used (26, 27). The heat that is produced is assumed to be due, in part, to the energy requirements involved in nerve conduction, but more largely to the processes that are associated with recovery from the preceding period of activity. Very recent investigations concerning changes that are brought about in the autonomic nervous system during activity are the basis for new pronouncements regarding chemical mediation in nerves (25, 46). Acetylcholine has been found to be produced in autonomic nerves during activity, not only at the synapse, but also at the neuronal surfaces. The characteristics of acetylcholine metabolism in nerve are such as to warrant the assumption that this ester may be the vehicle of nervous transmission.

The diverse changes in afferent nerves during stimulation are functionally antecedent to and continuous with changes in the prevailing state of the related parts of the central nervous system. The arrival of a large number of frequencies over a large number of afferent paths serves to induce a temporary change in the excitatory state of the spinal nerve centers. Although the nature of the changes which are produced in the spinal centers, *e.g.*, when a weight is impressed against a muscular system, has not as yet been so definitely revealed as those which take place under the same conditions in peripheral nerve, some notion regarding the nature of the changes in the centers may be inferred on the basis of existing neurophysiological and behavioristic evidence. The assumptions are designed to provide a basis for the explanation of the major characteristics of the phenomenon of post-contraction.

The specific mechanism underlying post-contraction may be assumed to comprise the functional changes that are set up in the spinal nerve centers during the antecedent (voluntary) stimula-

tion. The total input of frequencies over the afferent nerves builds up a physiological gradient in the spinal centers. Psychophysical information enables us to infer that the level (concentration) of the physiological gradient is a function of definite quantitative (intensive, temporal, areal) laws. Since a stronger stimulus will be attended by a larger total frequency input per unit time than a weaker stimulus, the rate at which the physiological gradient is built up in the spinal centers will be greater in the former than in the latter case. Likewise, when a given stimulus is allowed to act longer, the level that the physiological gradient attains will usually be higher than when the same stimulus is allowed to act for a shorter period.

The altered physiological conditions developed in the spinal centers set off impulses over the efferent neurones as soon as the voluntary contraction for a post-contraction ends, provided the arm is then permitted to assume the relaxed state that is usually prescribed for the appearance of a post-contraction (21). The output of impulses over the efferent paths in turn also is governed by definite quantitative laws. The higher the level of the temporary physiological state existing in the spinal nerve centers, the greater will be the total number of frequencies which are discharged per unit time, and accordingly the greater will be the number of muscle spindle cells that are brought into contraction.

Today, the test of this neurophysiological conception is its adequacy to explain the generally known facts of post-contraction. Tomorrow (we hope), the test will be quantitative agreement. Only the general characteristics of the phenomenon can be undertaken here.

The magnitude of post-contraction has repeatedly been observed to be greater for a more intense than for a weaker (voluntary) contraction. This is owing to the fact that, in the given period of stimulation, the physiological gradient that is induced in the spinal centers rises to a higher level for an intense than for a weak stimulation, because (1) a larger number of afferent nerves is probably called into action by the stronger stimulus, and (2) the input of frequencies over the afferent paths is greater per unit time throughout the voluntary stimulation for a stronger than for a weaker contraction. Accordingly, when the arm is allowed to relax at the end of the voluntary contraction so that the response may be exhibited, the output of frequencies from the spinal centers over efferent nerves to muscles normally may be expected to be greater for the more intense than for the weaker stimulation. A greater output of frequencies brings into contraction a larger number of spindle cells in the muscles of the arm, and the arm is elevated higher following a stronger stimulation.

A more intense stimulus also normally occasions a response which persists longer than a weaker one. Owing to the fact that a longer period of time is required for the dissipation of a higher than for a lower physiological gradient, the more intense stimulation maintains a transmission of impulses sufficient to support the weight of the arm for a longer period of time than a weaker one following the termination of the voluntary contraction. The duration and magnitude of the response need to be related quantitatively to the duration and intensity of the stimulus.

Post-contraction has been elicited contralaterally as well as ipselaterally because the impulses which are discharged from the spinal centers secure outlet not only over ipselateral efferent neurones but also *via* commissural fibers over efferent neurones on the opposite side.

Several well-known effects following

prestimulation, now classic, are also to be explained. After a unilateral prestimulation, *e.g.*, lifting a weight by one hand, the subject in making weight equations no longer pulls (approximately) equally with the two hands on a double spring-balance, but pulls harder with the hand that had been employed in the prestimulation (33, 34). Owing to the presence of a unilateral physiological gradient, many of the nerves which normally are involved in the act of lifting on that side are already 'busy.' At any given moment then, a substantial number of the nerves are in a somewhat prolonged refractory period and are therefore less efficient in transmitting impulses; hence, at a given moment, *e.g.*, when an equality judgment is being made, a greater pull will be required on the prestimulated side than on the 'rested' side to occasion the eventuation of a similar number of proprioceptive impulses from each side in the somesthetic cortex.

The instance of 'motor adaptation' which Müller and Schumann (36) and Steffens (56) studied, and which Myers (37) describes, has its basis in similar neurophysiological conditions. The heavier of two weights can be made to appear the lighter of the two in a bimanual comparison provided the weights are compared just after a prestimulation (lifting a weight) is given on the side which is used to lift the heavier weight. Immediately following the prestimulation, a substantial number of nerves on that side are temporarily less capable to transmit impulses due to prolonged refractoriness; the consequence is that in the subsequent comparison the total number of frequencies which is able to reach the somesthetic cortex from the prestimulated side due to lifting the heavier of the two weights is greatly reduced: in fact, the total number of impulses induced in the somesthetic cortex by the heavier weight from the muscles on the prestimulated side evidently is now less than the total number of impulses induced in the cortex by the lighter weight from the muscles on the other side.

Fechner's negative sensation has a similar neurophysiological meaning. The lighter weight appears 'negative' (much lighter than usual) when lifted immediately following a period in which the arm has been employed in lifting heavy weights because many of the afferent nerves possess prolonged refractory periods due to the preceding period of intense stimulation. Consequently, comparatively few proprioceptive impulses are now able to be transmitted to the somesthetic cortex. The resulting reduced excitation thus underlies the negative sensation.

These details concerning post-contraction have broader implications. They serve to illustrate the way in which psychophysical information may be employed as an aid to the study of the neurophysiology underlying a given item of behavior. Psychophysical studies comprise correlations between quantitative changes in the stimulus and quantitative aspects of behavior. However, a full account regarding the nature of a given item of behavior includes both the specification of the changes that are produced by the stimulus *immediately* in the organism and *more remotely* in behavior.

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ELEANOR HARRIS ROWLAND WEMBRIDGE

1883–1944

Eleanor Wembridge was a woman distinguished, in the finest sense of the word, both in her career and in her contributions to the field of psychology.

She was born in Lee, Massachusetts, her mother being a woman of high mental calibre and famous for good works, and her father the much loved and respected minister of the Congregational Church. She earned the Ph.D. from Radcliffe at an exceptionally early age, and came to Mount Holyoke College for her first experience as a college instructor. Here she served for five years in the Department of Philosophy and Psychology. Among her earliest publications were *Studies in the Esthetics of Visual Forms*, and an essay on certain aspects of the psychology of language. But her interests showed a gradual development from these experimental and abstract studies of the laboratory towards a series of researches in the more directly social and practical aspects of her field.

Her first book, *The Right to Believe*, shows the influence of her teacher William James, yet is independent in treatment and somewhat different in point of view from his *Will to Believe*. Her second book, *The Significance of Art*, proved to be a work of general interest, which was also used and valued as a college text in classes in aesthetics.

Dr. Wembridge's second collegiate post took her to the Pacific coast, where she became Dean of Women at Reed College in Portland, Oregon. Here she remained for four years doing pioneer work in the introduction of mental tests and in formulating policies and inaugurating practices in the administration of this institution.

Her marriage to Mr. Harry Wembridge was followed by a return to the east and to an association with the Intelligence Department of the Army, her husband being stationed at Fort Bard in that service. Soon after 1920 they came to Cleveland, and Mrs. Wembridge took charge of psychological work for the Women's Protective Association of that city. She was subsequently appointed by the County Commissioners to be referee of the Women's Department of the Juvenile Court. She filled this very responsible position in such a way as to win international reputation. She lectured extensively in this country during the nine year period of her Cleveland services, and published articles in American and in British periodicals. Well known are *The Morals of the Unmoral* and *The People of Moronia*. Among the books which make use of the material gathered in her social experiences are *Life Among the Low Brows* and *Other People's Daughters*. She also produced plays. One entitled *The First of May* received wide recognition and was presented repeatedly. Her last book, *Let's Understand Each Other*, gives the note of her purpose in many of these writings. She wished to present in a clear and telling way the great variety of human difficulties and human qualities with which she had come in contact. She succeeded eminently well. Without departing in any way from the strictest scientific regard for the psychological facts, she achieved literary quality. In a prose which is lucid and crisp she conveys material of deep human significance.

The last years of her life were spent in Los Angeles where she was active in

social work and in literary production. In evident recognition of her experience and her judgment, she was appointed in 1943 to serve on the Grand Jury of Los Angeles County.

Eleanor Wembridge was a woman of impressive achievement. She was gracious and generous in friendship—a spirit noble and humane.

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